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THE

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FOR OCTOBER 1861.

ART. I.—*Clinical Report on the Treatment of Phthisis by the Chlorate of Potassa.* By AUSTIN FLINT, M. D., Professor of the Principles and Practice of Medicine in the Bellevue Hospital Medical College, New York, and in the Long Island College Hospital.

AT the meeting of the American Medical Association in June, 1860, the late Dr. E. J. Fountain, of Davenport, Iowa, submitted to the Section on Practical Medicine, a paper on the Treatment of Phthisis by the Chlorate of Potassa, in conjunction with Remarks on Ozone and Oxygen as Therapeutic Agents. The paper was listened to with interest, but the members of the Section declined to recommend it for publication in the *Transactions*, on the ground that, so far as the use of the chlorate of potassa in phthisis is concerned, the cases reported were few (three or four only), and the diagnosis was not based on sufficiently complete evidence afforded by physical signs. The members of the Section were unwilling that the Association should appear to sanction the introduction of a new remedy claiming to exert a special influence over phthisis, unless this claim seemed to rest on abundant facts. A resolution was, therefore, adopted referring the paper back to the author, with the request that he pursue his investigations, and report at the next meeting of the Association. This action was acceptable to Dr. Fountain, and he was earnestly engaged in pursuing further experimental researches on the effects of the chlorate of potassa, when his labours in behalf of practical medicine were suddenly arrested by his untimely death. In a conversation with him at the meeting of the Association, I promised to make the employment of this remedy in phthisis the subject of clinical observation, and to furnish him with a statement of the results. I have not been unmindful of this promise, and I propose to present in this article the facts which I have recorded, and which I should have placed at his disposal had his valuable life been spared.

After the meeting of the Association, I prescribed this remedy in several cases of phthisis at my clinic at the Long Island College Hospital. It was not convenient for me to make record of these cases, and the patients were under observation for a short period only, but the impression which I received was favourable to the utility of the remedy. On entering upon duty at the New Orleans Charity Hospital, November, 1860, I adopted the following plan: Whenever cases of phthisis were admitted into my wards, the patients were at once, or soon after admission, placed under the use of this remedy, and it was continued so long as I was satisfied that they were not losing ground. When I became satisfied that they were losing ground, the remedy, as a rule, was discontinued. While the patients were taking the chlorate of potassa, I prescribed no other remedy, excepting the syrup of morphia[1] as a palliative of cough. I excluded alcoholic stimulants when I thought I could do so with propriety, but, in some cases, I did not feel at liberty to withhold these, and they were accordingly given while the patients were taking the chlorate of potassa. In all cases the full diet of the hospital was allowed, and the patients were encouraged to be up, and to go out of doors whenever the weather and their strength permitted.

In observing the apparent effects of any remedy in patients received into hospital with phthisis, the following points are to be kept in mind. The change in certain cases will alone be sufficient for a marked improvement. This will be likely to be manifest when the comforts of a hospital are exchanged for insufficient food, miserable lodgings, and the hardships incident to drunkenness. The expectation of benefit is sometimes not without influence. On the other hand, in some cases the change is for the worse, the condition as regards diet and accommodations being inferior to that before entering the hospital; moreover, the hygienic influences of the hospital from overcrowded wards, etc., may be positively pernicious. Some persons, too, receive an unfavourable mental influence from association with the sick and dying. The characteristic mental state in phthisis, as regards sanguine hopes of recovery, leads patients to persuade themselves for a time that any remedy which they are taking is highly beneficial, and the physician must, therefore, rely upon his own observation, rather than on the patients' statements for the evidence of improvement. On the other hand, patients affected with this, as well as any other chronic malady, are apt to become tired of any remedy if continued long, and they may be led to think they are not improving by a desire for a change in treatment.

I kept records of all the cases of phthisis received into my wards (containing about forty beds) during my winter's service, extending over nearly five months. To report the cases in full, would extend this article too much, and render it needlessly tedious. I shall give a report of each case separately, but confine myself to facts sufficient to show the correctness of

[1] The syrup of morphia of the hospital contains a grain of the sulphate of morphia in an ounce of simple syrup.

the diagnosis, the amount of tuberculous deposit, the existence, or otherwise, of important complications, the general condition of the patient when admitted, and the progress of the disease while the patient was under observation, with reference to the influence of the remedy under consideration. Under the head of *remarks*, appended to each case, I shall offer such statements as a reviewal of the case may suggest. Finally, under the head of *general conclusions*, I shall present a *résumé* of certain facts, together with such inferences as a survey of the apparent results in all the cases will warrant.

CASE 1. Michael Holligan, aged 50, Irishman, tanner, admitted Oct. 23, 1860.

Cough and expectoration had existed for three years. He was compelled to quit work only a month or so before his admission, but for several months he had been able to work only a portion of the day. He was accustomed to work within doors, and was much exposed to wet.

Moderate diarrhœa had existed much of the time for the preceding three years.

When admitted he was able to be up and dressed most of the day. He was considerably emaciated. The aspect was not notably morbid. He was deficient in breath on exercise. The cough was frequent and expectoration large. Appetite good. No diarrhœa. Pulse 96; respirations 20.

Physical Signs.—Flattening and diminished superior costal movement on the right side. Notable dulness on percussion between 2d and 5th rib on the right side, and well-marked cracked metal resonance. The respiration in the situation just named was broncho-cavernous, the cavernous predominating, accompanied with gurgling.

Cod-liver oil was at first prescribed, but diarrhœa occurred, and this remedy was discontinued. The bismuth was given with reference to the diarrhœa. On Nov. 3, the chlorate of potassa was prescribed, three drachms to be taken daily. Two ounces of brandy were given three times daily.

Nov. 11. The chlorate of potassa was discontinued on account of diarrhœa, and opium and tannic acid prescribed. Two days afterwards, the bismuth was substituted, and the syrup of morphia given to allay cough.

15*th.* No diarrhœa, and the chlorate of potassa was resumed in the same quantity as before, viz., three drachms daily.

Dec. 16. Under this date is the following record: "The treatment is continued. The patient appears to hold his own. If he fails, the failure is very gradual. He reports that he thinks he is better than when my service commenced. He is up and dressed all day. The diarrhœa of late has not been troublesome, but he complains of abdominal pain often. The abdomen is not tender to the touch. The expectoration is large, but he thinks has not increased. The appetite is tolerable. To-day, over the whole of the middle third of the right side of the chest, the respiration is bronchial, and accompanied with coarse mucous or cavernulous rales."

19*th.* The patient reported not so well. He complained of dyspnœa, and difficulty of expectoration. His aspect denoted failure since the date of the preceding record. The chlorate of potassa was discontinued.

24*th.* The patient had failed rapidly, and died on this date.

The lungs were removed for examination, and inadvertently lost. A superficial examination showed advanced tuberculosis, solidification, and

cavities. A large excavation, near the apex of the right lung, was opened in removing this lung from the chest.

Remarks.—This was certainly an unpromising case for the trial of any remedy. The pulmonary affection was extensive, far advanced, and probably complicated with intestinal tuberculosis. But for a month, while taking steadily three drachms of the chlorate of potassa daily, the failure was almost imperceptible. The patient kept up as long as possible, and when obliged to take to the bed, he sank very rapidly. We see this not infrequently in cases of pulmonary tuberculosis. Viewing this case alone, it is difficult to say how much importance belongs to the use of the remedy, or whether or not it exerted any beneficial effect. It is worthy of note, that the patient was able to take the remedy without any aggravation of the diarrhœa which had existed much of the time for three years.

CASE 2. Arthur Burns, aged 25, clerk, admitted May 3, 1860.

Two weeks before his admission he became affected with incomplete paralysis of the lower limbs. About the same time he began to cough. The paralysis of the limbs increased until he was unable to get about, and this led him to enter the hospital. Since his admission up to the commencement of my service (six months) he had kept the bed most of the time, but was able to walk about a little with the aid of a crutch and cane. Cough and expectoration had progressively increased since his admission.

When the record was commenced, Nov. 2, 1860, he was greatly emaciated and intensely anæmic. The appetite was poor, and the bowels occasionally loose. Pulse 96 and very small; respirations 12. He estimated the quantity of expectoration at about a pint daily. He kept the bed constantly.

Physical Signs.—Diminished superior costal movement and dulness on percussion at the summit, were marked on the left side. Over a circumscribed space as large as a dollar, the respiration was cavernous (the inspiration non-vesicular and low, and the expiration lower in pitch than the inspiration). Below this space the respiration was broncho-vesicular. The cavernous whisper was marked within the space in which the respiration was cavernous.

The chlorate of potassa was prescribed a week before the record was commenced. It was borne well, three drachms being given daily. Three ounces of brandy were given daily, and the syrup of morphia to allay cough.

The patient reported that he had greatly improved since this treatment was adopted, but his general appearance did not denote any improvement.

Nov. 18. The patient had evidently failed, but he daily reported better, and complained of nothing. The chlorate of potassa was continued with brandy and the syrup of morphia.

20*th.* Death occurred on this date. The patient continued to report better, and apparently died under the delusion that he was progressively improving. Diarrhœa was troublesome during the last few days of life, and the evacuations were frequently passed in bed.

On examination after death, at the apex of the left lung a cavity was found nearly as large as a small orange. Small deposits of softened tubercle existed in the neighborhood of the large cavity, but not enough to produce much consolidation. There were no collections of crude tubercle, and

no cavities existed near the large excavation. At the lower part of the upper lobe were two small cavities of the size of filberts. A cavity of the size of a hickory nut existed at the posterior and inferior portion of the upper lobe of the right lung. Smaller cavities, and small collections of softened tubercle existed in this lung.

The intestines presented no morbid appearances. The bladder was contracted, the lining membrane thickened, and its surface covered with bloody mucus.

Remarks.—This case also was extremely unpromising. The chlorate of potassa given for nearly a month exerted no apparent effect to retard the progressive failure of the powers of life ending in death.

CASE 3. Patrick Corley, aged 34, labourer, admitted Oct. 3, 1860.

He stated that he had had cough and expectoration for two and a half years. During this period he had received no medical treatment, and he had not quit work even for a day until he entered the hospital. He entered for dengue, which was then very prevalent. He was confined to the bed for several days with the dengue. On recovering from that affection, he found himself weak, and the cough and expectoration had increased.

When my record commenced, Nov. 5, 1860, he was up all day, and much of the time out of doors. He estimated the quantity of expectoration at half a pint daily. The aspect was pallid, but he did not appear to be much emaciated. He thought he was twenty pounds under his average weight in full health. Pulse 88; respirations 24.

Physical Signs.—Flattening at the summit of the right side of the chest, with diminished superior costal movement, and restrained elevation of the right scapula on forced breathing. Dulness on percussion at the right summit marked, the respiration broncho-vesicular, and relatively feeble.

The treatment for the month he had already been in hospital was cod-liver oil, with brandy and full diet. The cod-liver oil was discontinued Nov. 5, and three drachms of the chlorate of potassa, daily, substituted. An ounce of brandy was given three times daily, and the syrup of morphia to allay cough.

Dec. 10. The patient on this date reported that he felt well enough to leave the hospital, and undertake to work as a labourer. He stated that he had gained steadily in strength since my treatment was commenced, and that his cough and expectoration had considerably lessened. His aspect was improved, but he was still pallid. He was discharged on this date.

Remarks.—The previous history, and the physical signs in this case, show that the tuberculous deposit was of long standing, and that the affection was not progressive, but probably retrogressive. The case illustrates facts which I have repeatedly observed, viz., the cachexia may be, as it were, exhausted by a certain amount of deposit, and the disease tend intrinsically to pursue a favourable course. Although undoubtedly tuberculous for two and a half years, the patient had kept steadily at work, and the disease remained in abeyance without medical treatment. The facts illustrated by this case are to be considered in endeavouring to estimate the influence of remedies. This patient entered the hospital not for the tuberculosis, but for dengue. He was left enfeebled by the latter, and when he

had recovered the condition of health in which he was at the time of being attacked with that affection, he was as able to work as he had been previously. It may fairly be doubted whether this case furnishes evidence of any positive influence of the chlorate of potassa on the tuberculous disease.

CASE 4. Daniel McFaden, aged 22, Irishman, labourer, admitted Nov. 7, 1860.

Sixteen months before his admission he had a copious hæmoptysis. After this he was well apparently for four months, when cough commenced. Afterwards he had several attacks of hæmoptysis. He continued to work until shortly before he entered the hospital, but for some time he had not been able to perform full days' labour.

At the time of his admission he was twenty-seven pounds under his average weight in health. He was able to be up all day and out of doors. His aspect was not notably morbid; the prolabia presented a good colour. The expectoration was large. The appetite was good, and the bowels constipated. Pulse 100; respirations 32.

Physical Signs.—Diminished superior costal movement on the right side, and marked dulness on percussion. In front, on this side, the respiration almost inappreciable and accompanied by subcrepitant rales; behind the respiration broncho-vesicular.

Half an ounce of the chlorate of potassa daily was prescribed, and the syrup of morphia.

Dec. 2. Under this date the following record was made: "This patient of late has daily reported an increase of cough and expectoration, and decrease of strength. He states to-day that he has lost eight pounds since he entered the hospital. He has evidently lost ground, although he is still up and about all day. Concluding, therefore, that I cannot conscientiously continue longer the chlorate of potassa treatment, I have to-day directed its discontinuance, and have prescribed half an ounce of the cod-liver oil, and an ounce and a half of brandy, three times daily."

3*d.* The patient desired to leave the hospital, and was discharged.

Remarks.—In this case the amount of tuberculous deposit was not very large, and there was no important complication. The chlorate of potassa, which, with the syrup of morphia, constituted the treatment for nearly a month, half an ounce being given daily, failed to prevent an increase of the cough and expectoration, with progressive failure of strength, and the patient was sufficiently discouraged to request a discharge from the hospital.

CASE 5. Timothy Ford, aged 45, Irishman, labourer, admitted Nov. 6, 1860.

A year before his admission he had an attack of hæmoptysis, but he had no cough, and was apparently in good health, until March or April, 1860. He continued to labour until July, and was then obliged to quit work on account of weakness.

At the time of his admission he was up all day and able to be out of doors. The quantity of expectoration was considerable. The appetite was tolerable. The bowels were constipated. Pulse 100, small and feeble; respirations 28.

Physical Signs.—Diminished superior costal movements on the right side, and notable dulness on percussion in the infra-clavicular region. The

respiration in this region extremely feeble, and accompanied by mucous rales. Vocal resonance intense in this region, and feeble on the left side. Bronchial whisper more acute than on the left side. Behind, relative dulness over the right scapula; the respiration inappreciable in the upper scapular, and broncho-vesicular in the lower scapular region. Equal resonance and respiration below the scapulæ on the two sides.

Treatment.—Half an ounce of the chlorate of potassa daily, and the syrup of morphia *pro re nata.*

Nov. 20. The patient desired to leave the hospital, and was discharged on this date.

He reported on this date that for two or three days the cough and expectoration had been less than usual. His condition, however, when discharged appeared to be about the same as when admitted.

Remarks.—The chlorate of potassa given to the extent of half an ounce daily in this case for fourteen days, constituted, with the syrup of morphia, the whole medical treatment. It exerted no marked influence on the pulmonary symptoms, or the general condition.

CASE 6. Silas Worden, American, plasterer, aged 41, admitted Nov. 13, 1860.

He had good health up to July, 1860. He was then attacked with an acute affection which was called swamp fever, and confined to the bed for several weeks. He recovered sufficiently to go to work, and was then seized with the dengue. After recovering from this affection he was attacked with intermittent fever, which was arrested after three paroxysms. About eight weeks before his admission he began to cough. The cough was at first slight and the expectoration small. These symptoms increased, and he lost, progressively, strength and flesh until he was too weak to work, and came to the hospital. He had a slight hæmoptysis three weeks before his admission.

At the time of his admission he was able to be up all day and out of doors. The appetite was good and the bowels regular. He was thin but not notably emaciated. The aspect was pallid. The pulse was 100, small and feeble; the respirations, 24.

Physical Signs.—Diminished superior costal movements on the left side. Marked dulness on percussion at the left summit in front and behind; the respiration broncho-vesicular, approaching the bronchial, the voice somewhat bronchophonic, and the bronchial whisper notably intense and acute.

Treatment.—Half an ounce of the chlorate of potassa daily, and the syrup of morphia *pro re nata.*

Dec. 3. The patient had shortly prior to this date a slight hæmoptysis. The cough and expectoration were increased. He had lost in weight and was more anæmic than on the previous date.

The treatment was continued.

10*th.* The patient complained of profuse sweating at night. Treatment continued.

22*d.* The increase in the pulmonary symptoms and the loss in weight and strength were such that I did not feel warranted in continuing longer the chlorate of potassa treatment, and it was on this date discontinued. Cod-liver oil and brandy were prescribed. The patient was still able to be up all day.

Feb. 8, 1861. The patient steadily declined after the preceding date and died on this day.

On examination after death the left lung was found to contain, near the apex, a cavity as large as an English walnut, and the upper and middle lobes were solidified by tuberculous deposit. A small cavity existed at the apex of the right lung, with considerable tuberculous solidification in the upper third of this lung.

Remarks.—The chlorate of potassa treatment was commenced in this case eight weeks after the occurrence of cough denoted the deposit of tubercle. The deposit was abundant when this treatment was commenced. The treatment was continued for six weeks and then abandoned in consequence of the steady progress of the disease and with loss of strength and weight during that period.

CASE 7. Hugh Ramsey, Scotchman, aged 31, bookkeeper, admitted Nov. 19, 1860.

In January, 1860, he had measles, and had not been free from cough since that time. In the month of March, 1860, he had slight hæmoptysis, recurring two or three times. The voice had been constantly husky for five or six weeks. He had recently come to New Orleans from Winchester, Virginia.

At the time of his admission he was thirty-four pounds below his average weight in health. He had left the bed for several days. The appetite was good. No diarrhœa. Pulse 96; respirations 32. The expectoration was copious.

Physical Signs.—Moderate relative dulness on percussion at summit of left side, in front and behind; the respiration broncho-vesicular; the voice bronchophonic, and the bronchial whisper intense and acute.

Treatment.—Half an ounce of the chlorate of potassa daily, with the syrup of morphia *pro re nata*.

Nov. 29. The patient was discharged at his request, being desirous to get to some friends in Maryland. During his stay in the hospital, the cough and expectoration diminished, and his appearance denoted improvement. The voice remained the same.

Remarks.—This case was under observation for ten days only. The condition of the patient was improved, but how far the improvement was due to the chlorate of potassa, irrespective of the influence of rest and change of climate, it is difficult to say.

CASE 8. James Ryan, aged about 21, labourer, admitted Dec. 8, 1860.

Cough had existed for a year. He continued to work up to November, and was then obliged to quit work on account of weakness and want of breath. He had lost considerably in weight.

On admission he was able to be up all day and out of doors. The expectoration was pretty large. The pulse was 64; the respirations 20. He had diarrhœa, which had existed for two days only.

Physical Signs.—Diminished superior costal movements on left side, and elevation movement of left scapula on forced breathing restrained. In front, at summit of left side, the percussion sound vesiculo-tympanitic. Distinct dulness over left scapula. Vocal resonance greater at the left than

at the right summit. The voice over the left scapula slightly bronchophonic.

Treatment.—The oxide of bismuth was at first prescribed for diarrhœa, and this symptom being relieved, half an ounce of the chlorate of potassa *per diem* was prescribed Nov. 10, with the syrup of morphia *pro re nata*.

Dec. 29. A slight hæmoptysis occurred. The treatment was continued.

Jan. 1, 1861. The patient having complained of late of an increase of the cough and expectoration, and reporting that he felt progressively weaker, the chlorate of potassa was suspended, and half an ounce of cod-liver oil, with an ounce of brandy, three times daily, prescribed.

9*th*. A small hæmoptysis occurred. The cod-liver oil and brandy were continued.

Feb. 11. The patient for some days had reported not so well—more cough and expectoration, and greater debility. The general appearance did not show deterioration, and he continued to be up all day and out of doors. On repeated examinations of the chest, no evidence of an increase of the deposit of tubercle was obtained.

On this date the chlorate of potassa was resumed, half an ounce daily, and the brandy continued.

March 11. The patient reported that he felt well enough to go out, and undertake to work. He was accordingly discharged. The chlorate of potassa was continued up to the date of his discharge. He stated that the cough and expectoration were, at least, one-fourth less than when he entered, and his strength about the same as at that time. The physical signs showed no increase of the deposit during his stay in hospital.

Remarks.—The chlorate of potassa was given in this case for three weeks; cod-liver oil and brandy then constituted the treatment for six weeks, and for the remaining four weeks, up to the date of his discharge, the chlorate of potassa was resumed. The improvement occurred during the latter period, and in connection with this fact the change of season is to be considered, the weather having become warm. Moreover, the ward was not as crowded as during the two former periods.

It is also to be considered that, after resuming the chlorate of potassa, the brandy was continued, which was not given during the first period. In view of these circumstances, it is difficult to judge of the agency of the chlorate of potassa in the improvement.

CASE 9. John Cock, aged 33, cooper, German, admitted Dec. 17, 1860. Cough had existed for about twenty weeks. He had lost, as he thinks, thirty pounds in weight during this period.

At the time of his admission, he was able to be up and dressed all day, but he was pallid, emaciated, and appeared to be quite feeble. The pulse was 56 and very small. The respirations were not accelerated. Appetite poor. Bowels constipated.

Physical Signs.—Deficient superior costal movement on the left side of the chest, with notable dulness on percussion, and broncho-vesicular respiration, approaching to bronchial, exaggerated bronchial whisper, and the vocal resonance and fremitus greater than on the right side.

Treatment.—Half an ounce of the chlorate of potassa daily, with syrup of morphia *pro re nata*.

Dec. 29. The cough and expectoration had somewhat diminished, but

he had not improved in appetite and strength. The chlorate of potassa was continued, and an ounce of brandy, three times daily, added.

Jan. 10, 1861. The patient had declined in strength, and now kept the bed constantly. The pulse was extremely feeble. The expectoration of late had been abundant and purulent. The cavernous respiration and whisper were marked in the left infra-clavicular region, over a space covered by the pectoral extremity of Cammann's stethoscope.

The chlorate of potassa was continued, with brandy every four hours.

Death occurred January 11.

On examination after death, a large cavity was found at the upper part of the left lung, and in the vicinity crude tubercle existed in abundance. A few small collections of softened tubercle were observed. The upper lobe of the right lung was crammed with tubercles, not aggregated into masses, nor softened, and no cavities.

Remarks.—In this case the chlorate of potassa, given during the time the case was under observation (twenty-four days), did not prevent the rapid progress of the disease, and its fatal termination. During this time the tuberculous affection in the left lung eventuated in the formation of a large cavity, and a fresh deposition of a large amount of tubercle took place.

CASE 10. James Burns, Swede, aged 36, labourer, admitted Jan. 2, 1861.

Cough had existed for four months. He was obliged to quit work three weeks before his admission, on account of weakness and deficiency of breath.

At the time of his admission, he was able to be up all day and out of doors. His aspect was not notably morbid. He was moderately anæmic. The pulse was 100 and small; the respirations 24. He was twenty-five pounds below his average weight in health. The appetite was good; the bowels constipated.

Physical Signs.—Depression at the summit on the right side; diminished superior costal movements on that side, with notable dulness on percussion; broncho-vesicular respiration; the vocal resonance and bronchial whisper more intense than on the right side.

Treatment.—Half an ounce of the chlorate of potassa daily, and the syrup of morphia *pro re nata.*

Jan. 17. The patient reported that the cough and expectoration were about the same as when he entered, but that he had failed as regards appetite and strength. It was apparent that he had lost ground, and on this date the chlorate of potassa was discontinued, cod-liver oil and brandy being substituted.

22*d.* Physical signs showed marked increase in the amount of tuberculous deposit, and the existence of a cavity in the upper part of the right lung.

Death occurred February 1.

On examination after death, a tuberculous cavity was found at the apex of the left lung, nearly as large as a medium sized orange. The whole of the right lung was solidified with crude tubercle. The left lung contained a cavity as large as an English walnut, and portions of the upper lobe were solidified with crude tubercle.

Remarks.—The chlorate of potassa in this case, given for fifteen days, did not prevent the rapid progress of the tuberculous affection, nor a fresh deposition of tubercle, ending in death on the twenty-ninth day after admission.

CASE 11. Edward Flynn, aged 28, Irishman, labourer, admitted Dec. 25, 1860.

A copious hæmoptysis occurred a year before his admission. Two months after this, cough and expectoration became prominent symptoms, and he was obliged to quit work seven months before his admission.

At the time of his admission, he was able to be up all day and out of doors. He thought he was twenty-five or thirty pounds below his average weight in health. He presented an emaciated appearance, and was notably anæmic. The appetite was poor. The expectoration was not large. The pulse was 120, and quite small; the respirations 36.

Physical Signs.—Notable depression at the right summit, and dulness on percussion; a feeble broncho-vesicular respiration in front, accompanied by subcrepitant rale; behind, in lower scapular region, the respiration nearly bronchial; the vocal resonance intense in front.

Treatment.—Half an ounce of the chlorate of potassa daily, and syrup of morphia *pro re nata.* An ounce of brandy three times daily was also given.

Jan. 21. The patient was discharged at his request. The cough and expectoration had increased since his admission, and he had failed as regards strength. His general appearance was worse, and I have noted, in connection with the history of the case, that had he remained in hospital I should have discontinued the chlorate of potassa at the date of his discharge.

Remarks.—The chlorate of potassa, given in this case for nearly four weeks, failed to produce any improvement.

CASE 12. Gottleib Knaonar, German, butcher, aged 42, admitted Dec. 25, 1860.

Cough and expectoration had existed for thirteen months. Hæmoptysis had occurred. In other respects the previous history was imperfectly obtained.

At the time of his admission he kept the bed, and complained much of pain in the right side. I insisted upon his getting up, and in a short time he remained up all day, and was soon able to act as an assistant to the ward nurse. The pulse was 112; respirations 32. The expectoration was small. The aspect was not notably morbid. The appetite was good.

Physical Signs.—Dulness on percussion marked in the right infra-clavicular and lower scapular regions; broncho-vesicular respiration approximating to the bronchial in these regions, and bronchophony.

Treatment.—Chlorate of potassa, half an ounce daily, was prescribed Jan. 1, 1861, and an ounce of brandy three times daily.

Jan. 28. The following is the record under this date: "On this day the chlorate of potassa is discontinued and cod-liver oil, half an ounce three times daily, substituted, the brandy being continued. The patient for some time has complained of an increase of the cough and expectoration, and of loss of strength. He is evidently not improving, if he be not losing ground. He states that he has formerly been benefited by the cod-liver oil. Under these circumstances, having concluded that I have given the chlorate of potassa a fair trial, I have changed the treatment."

Feb. 17. The patient reported better, and appeared to have improved since the foregoing date. The cough and expectoration had diminished, and he had more strength. The cod-liver oil and brandy were continued.

28*th*. The improvement was marked, and the patient had undertaken to discharge the duties of nurse in another ward.

Remarks.—The chlorate of potassa in this case, continued for twenty-eight days, produced no improvement. Subsequently, improvement was marked under the use of cod-liver oil. To the history of this case I have appended, in my hospital book, the following note: "The improvement in this case, it is to be considered, has been since the warm weather enabled patients to be out of doors, and secured for the wards free ventilation, the hospital, moreover, being less crowded."

CASE 13. Nelson Rasford, American, calker and carpenter, aged 37, admitted Dec. 26, 1860.

Cough had existed for five months. Hæmoptysis occurred a month before his admission. He had not worked for five months, and had been in a hospital at Smithland, Ky., for three months.

At the time of his admission he was able to be up all day and out of doors. He was thirty pounds under his average weight in health. The expectoration was abundant. He had diarrhœa, which had existed for a fortnight.

Physical Signs.—Dulness on percussion at the summit of the right side, in front and behind; the respiration broncho-vesicular; the bronchial whisper notably louder and more acute than on the left side, and the vocal resonance intense, but no bronchophony.

Treatment.—The oxide of bismuth was given for three days, with reference to the diarrhœa, which then ceased.

Dec. 30. Half an ounce of the chlorate of potassa daily was prescribed, and half an ounce of brandy three times *per diem*, with syrup of morphia *pro re nata*.

Jan. 19, 1861. The patient desired to leave the hospital, and was discharged. He proposed to try to do some light work. During his stay in hospital he had two attacks of hæmoptysis, the quantity of blood raised with each attack being small.

As regards his condition when discharged in comparison with his condition when he entered, the expectoration was about the same in amount, and he appeared to have neither lost nor gained in strength.

Remarks.—There was certainly no improvement in this case during the twenty days that the chlorate of potassa treatment was pursued.

CASE 14. Michael Murray, aged 27, hostler, admitted Dec. 3, 1860.

Cough had existed for three months. During this time he had lost moderately in weight, and considerably in strength.

At the time of his admission he was pallid and quite weak, but was able to be up all day and out of doors.

Physical Signs.—Distinct dulness on percussion at the summit of the left side of the chest, in front and behind; broncho-vesicular respiration; the bronchial whisper more intense as well as more acute than on the right side, and the vocal resonance slightly greater than on the right side.

Treatment.—Half an ounce of the chlorate of potassa daily.

Feb. 3, 1861. On this date the patient reported well enough to leave the hospital, and was discharged. The improvement had been progressive and was marked. The cough and expectoration were diminished one-half. He

appeared to have gained in weight. He was less pallid. The appetite and digestion were good. The physical signs were the same when discharged as when he was admitted.

Remarks.—The chlorate of potassa in this case was given for two months, and was the only remedy given. Under this treatment there was no evidence of progress of the tuberculous affection, and the improvement in all respects was striking, the patient recovering sufficient strength to feel able to return to labour.

General Conclusions.—It will not be questioned that the value of this or any remedy, in the treatment of this or any disease, is to be determined by experience. The only questions are, what are the conclusions to be drawn from experience, and how are they to be obtained? If the treatment of cases of phthisis by the chlorate of potassa be followed by recovery, or improvement, not attributable to other agencies, nor to the intrinsic tendencies of the disease, this remedy has a certain value. The degree of value is according to the ratio of cases in which it exerts a remedial influence, the proportion of instances in which it conduces to recovery, and the amount of improvement due to it when patients do not recover. Collections of cases in which the treatment consisted chiefly, or exclusively, in the employment of this remedy, contain the data for determining its value. A few cases are not sufficient, for it is certain that phthisis does not always tend intrinsically to a fatal result, but in some instances ends spontaneously in recovery. I have sufficient evidence of the correctness of this statement. Moreover, not very infrequently patients affected with phthisis remain for a long period in a condition nearly or quite stationary, the affection not progressing, and no fresh deposition of tubercle taking place, when this condition is not dependent on the influence of any remedy. I have records of cases which warrant this statement. In determining the value of this, or any remedy, therefore, the number of cases collected must be sufficient to make fair allowance for the facts just stated.

The cases should be recorded. To carry in the mind all the important facts, in a series of cases, having reference to the value of a remedial agent, is simply an impossibility. I do not mean to be understood as saying that the conclusions drawn from unrecorded experience are worthless. They are entitled to more or less weight, according to the estimate which we may place on the mental capacity and candour of those who communicate them, and the confidence which we may feel in our own faculties when the conclusions are our own. Therapeutical doctrines have always been, and must for a long period to come continue to be, to a greater or less extent, based on this kind of experience, when they are not purely theoretical, because the results of a better experience are acquired very slowly. This better experience consists in the deductions legitimately drawn from care-

fully recorded cases. In this way only can facts be accumulated in sufficient number for analytical investigation, and in this way only are the errors arising from preconvictions and hasty generalizations to be avoided.

The fourteen cases of phthisis now reported, were all that came under observation in my wards in the Charity Hospital during my last winter's service. I recorded these cases with express reference to the study of the influence of the chlorate of potassa on the disease, and, as already stated, I had previously been led to entertain the expectation that this remedy would be found to be valuable, from the cases reported by the late Dr. Fountain, and from having prescribed it in a few cases which were not recorded. In each of these fourteen cases the physical signs and symptoms afforded ample evidence of the existence of the disease. In five of the cases the disease proved fatal in the hospital, and the correctness of the diagnosis was confirmed by examinations after death.

The tuberculous disease had existed for some time in all the cases, and in most of the cases for a considerable period. This is a point to be considered. The question may fairly be raised, whether the results might not have been different in a series of cases in which the remedy was earlier employed. The facts with regard to the duration of the disease before the patients came under my observation are as follows: In one case it had existed over 3 years; in one case over 2 years; in three cases 1 year or over; in four cases 6 months or over; in two cases 5 months; in one case 3 months, and in two cases 2 months. In one of the two cases in which the disease had existed for only two months, the remedy was continued for six weeks without improvement, and then discontinued. This case ended fatally three months after the date of admission.

The quantity of tuberculous deposit is another point to be considered. In one of the cases only was the amount small. It was abundant in all the remainder, and in several cases it was large. The quantity of deposit may be considered as a criterion of the intensity of the cachexia, and in proportion to the quantity is the damage which the lungs sustain and the difficulty of recovery. It is therefore reasonable that in a series of cases in which a small deposit existed a remedy might be found to be efficient, which would be incompetent to effect a cure when the deposit is large.

Complications are of importance. For example, a coexisting tuberculous affection of the intestines diminishes the chances of recovery or improvement. This complication existed in one of the fatal cases. In another fatal case the patient had incomplete paraplegia and cystitis. In the other cases no important complications existed.

The condition of patients with this disease as regards muscular strength, is important with reference to the prospect of recovery or improvement. These cases differed in this regard, but in all the cases save one, the patients, when they came under my charge, were able to be up, either the whole or the greater part of the day, and to go out of doors.

The chlorate of potassa was given in most of the cases for several weeks. To be more precise, it was given for about a month in 6 cases; for two months in 1 case; for seven weeks in 1 case; for six weeks in 1 case; for twenty-four days in 1 case; for three weeks in 1 case; for two weeks in 1 case, and in one case for only ten days. The quantity given in the three first cases was three drachms, and in all the remaining cases half an ounce daily. A fresh solution was prepared each day and drank during the day. In all the cases the remedy was borne without inconvenience. It did not occasion diarrhœa nor any other unpleasant symptoms, and the patients did not seem to acquire a repugnance to it. In all the cases, while the treatment by this remedy was continued, other medicines were not given excepting the syrup of morphia as a palliative for cough, and in seven of the cases a small quantity of brandy.

Now, surveying these cases, what general conclusions are to be drawn respecting the influence of the remedy on the disease? In *nine* of the fourteen cases there are no grounds for supposing that the remedy exerted any salutary influence. In six of these nine cases the disease steadily advanced to a fatal issue, the patients dying in hospital. The histories of most of the fatal cases show that the remedy did not prevent the progress of softening, the formation of cavities, nor the fresh deposition of tubercle.

In *five* cases, a comparison of the condition of the patients before and after the use of the remedy, affords grounds for the supposition that the remedy may have exerted a salutary influence on the disease. These cases are Nos. 1, 3, 7, 8, 14. Reverting to these cases severally, in Case No. 1 the patient for a month, while taking the remedy, appeared to hold his own. He began, however, to fail rapidly while taking the remedy, so that it was discontinued five days before death. The evidence of the value of the remedy afforded by this case is certainly not strong.

In Case No. 3 the tuberculous disease had been of long standing, and was probably not progressive when the patient was admitted. He entered the hospital with dengue, and recovered from this affection with the tuberculous disease as it was previously. This case certainly affords no positive evidence of any influence exerted by the remedy on the tuberculous disease.

Case No. 7 was under observation only for ten days. The patient entered just after journeying from Virginia to New Orleans. He improved during his brief stay in the hospital, but there is at least as much ground for supposing that the improvement was due to rest and the change of climate as to the chlorate of potassa.

In Case No. 8 the chlorate of potassa was given for three weeks, and then discontinued in consequence of the want of evidence of improvement. Subsequently the remedy was resumed and continued for three weeks, and during this period the improvement was marked. In the mean time, however, the weather had become mild, and the sanitary condition of the hospital was improved by fewer inmates and freer ventilation. These circum-

stances render it doubtful whether much, if any, influence was exerted by the remedy.

In Case No. 14 improvement was more marked than in any of the other cases. This was the only case in which the quantity of tuberculous deposit was small. The disease had existed for three months only. The chlorate of potassa was the only remedy given in this case, and it was given during the whole time the patient was in hospital, viz., two months. The only question in this case is, whether the improvement was due to the intrinsic tendency of the disease, together with the sanitary influences of the hospital and season, or whether it was attributable, in part or exclusively, to the remedy.

In view of the facts presented in this report, the following conclusions are submitted:—

1. Of fourteen recorded cases of phthisis in which the chlorate of potassa was given in sufficient doses (ʒss *per diem* in eleven, and ʒiij *per diem* in three cases), and for a sufficient period to test its remedial power, in nine the histories afford no evidence of any salutary influence from the remedy; in four cases the circumstances render it doubtful whether much, if any, influence was fairly attributable to the remedy, and in one case only is there room for the supposition that the remedy was highly beneficial. These cases, therefore, fail to furnish proof of any special efficacy in this remedy to arrest or retard the progress of the disease.

2. Inasmuch as in all these cases, save one, the quantity of tuberculous deposit was large or abundant, and in nearly all the disease had existed for a considerable period, it remains to be ascertained by further clinical researches whether different results may not be obtained by the use of the remedy in a series of cases in which the quantity of deposit is small and the previous duration of the disease short. In collecting such cases, accuracy of diagnosis is, of course, essential, and this can only be secured by the evidence afforded by physical signs in conjunction with the previous history and present symptoms.[1] It may be added, in arriving at the conclusion that this remedy possesses no special influence in phthisis, it does not follow that it is not in a certain number of cases useful. By a special influence is meant a power to control, to a greater or less extent, the pathological processes which belong to this disease; a remedy may fail to do this, and yet be beneficial, as are a diversity of tonic remedies in cases of phthisis.

3. The chlorate of potassa may be given in cases of phthisis to the extent of half an ounce *per diem*, with entire impunity, and without occasioning any unpleasant symptoms. It does not produce diarrhœa, and may be well borne when diarrhœa is present in cases of phthisis.

[1] Case No. 6 is an instance, already referred to, in which the remedy was commenced two months after the commencement of the disease, and continued without improvement for six weeks.

ART. II.—*Gunshot Wound of Arm; Amputation at the Shoulder-Joint. Severe Effects to Operator from Inoculation with Matter from Wound.* By B. J. D. IRWIN, M. D., Assistant Surgeon United States Army.

JAMES NELSON, æt. 21, American, tall and well developed, with a healthy, robust constitution, received a severe gunshot wound early in the morning of March 4, 1861, while defending a house against a party of fifty or sixty Indians, who had surrounded it, and were endeavouring to destroy its four inmates by the medium of fire, which they had communicated to the building. He had been shot from a distance of fifteen or twenty yards, with a large bullet, which entered the flesh about one inch above the left nipple, and, impinging on the rib, glanced outward and backward, beneath the pectoral muscles, through a space of about four inches, after which it encountered the arm on its inner side, and passed through it, parallel to its original course, shattering the humerus at its upper third. The casual arrival at noon of myself and a brother officer, at the place, afforded means of succouring the party from their trying position. A hurried examination revealed the fact that considerable hemorrhage had taken place during the nine hours elapsed since the reception of the lesion, producing much depression, which was increased by the terrible prospect of impending death of a horrible character, which constantly presented itself for several hours. The limb I found already greatly swollen from sanguineous extravasation. After ordering cold water-dressing, to be applied to the wound, and making arrangements to have him taken to the military hospital, I left to accompany a detachment of troops, and did not again see him until the morning of the 6th. His arm and shoulder were then so much tumefied, that I found it impossible to discover the extent of the injury inflicted. Cooling lotions of cold water, containing chlorohydrate of ammonia dissolved therein, were constantly applied to the wounds, and half-grain doses of sulphate of morphia administered, whenever severe pain or restlessness indicated its use. On the 9th, suppuration having commenced, linseed-meal epithems were substituted for the water-dressing, and the wound kept clean with dilute liquor sodæ chlorinatæ. Next morning some symptoms of pleuritic inflammation were evinced over the anterior portion of the thorax. Six cut cups were applied, and an expectorant mixture containing one grain of antimonii tart. in four ounces, of which he was ordered a teaspoonful every hour, and which removed all symptoms by the following day. During the succeeding three days his condition showed no unusual symptoms. The discharge was profuse and fetid; the pus being mixed with large quantities of decomposed blood.

14*th*. The swelling having been reduced somewhat, I was enabled to examine the wound, and found the humerus splintered into numerous frag-

ments through about four inches of its shaft. It was evident that it would be impossible to save the arm; and any attempt to do so would be fraught with the most imminent danger, as the patient would most undoubtedly succumb to the prostrating effects of such an injury so soon as the malarial poison, ever present at this place, could wreak its baneful influence by producing diarrhœa, or its more fatal congener, dysentery.

After I represented the true condition of his case to the patient, he requested to have the limb removed, while he was yet in condition to bear up against the operation. At this time wine and generous nourishing diet were allowed him, and his condition promised a favourable termination.

16*th*. After inducing anæsthesia, by a mixture of equal quantities of sulphuric ether and chloroform, I amputated the limb at the shoulder-joint, making an external and internal flap. Some momentary difficulty occurred in disarticulating the head of the bone from the glenoid cavity, consequent upon the shortness of the upper fragment, which prevented the limb being used as a lever to assist in throwing the bone from its socket. A large quantity of fetid pus, mixed with grumous blood in a putrid condition, was found infiltrated throughout the tissues surrounding the joint and in the axilla. Only four vessels required ligation. The wound was brought together, and retained in apposition by interrupted sutures and strips of isinglass plaster, and cold water-dressing applied. Half a grain of morph. sulph. was given, and wine and brandy allowed *pro re nata*.

19*th*. Dressing removed; wound looks healthy; suppurating freely.

20*th*. Wound is united along the edges; patient doing well.

22*d*. Yesterday three ligatures came away, and to-day that from the axillary artery.

25*th*. Union by first intention has taken place throughout the wound, save at the angles, from whence a small amount of pus is discharging. ℞.—Infus. cinchonæ, ℥ss, ter in die.

26*th*. Had a severe attack of intermittent fever. ℞.—Mass. pil. hyd. gr. x. Fiat in pil. no. ij, to be followed by tinct. rhei f℥j; sulph. quiniæ gr. x, at bedtime.

27*th*. Wound looks very well, and is almost entirely healed. Repeated the quinia.

31*st*. Has continued improving until to-day, when he had another attack of intermittent fever, having its different stages well marked. Some tenderness over the liver and spleen. Repeated the blue mass and quinia.

April 2. No return of fever; is rapidly convalescing; able to walk about and take gentle exercise. ℞.—Infus. cinchonæ f℥j, three times daily, an hour before eating, and tinct. ferri chloridi gtt. xxx, morning and evening.

7*th*. Wound entirely united, save at two small points. Stump looks well. Is able to take exercise out of doors, and feels in excellent spirits. Complains of loss of appetite in the morning. Tinct. gentianeæ f℥ij, ter in die, substituted for the infusion of cinchona.

8th. While walking about at some distance from the hospital, was attacked with a severe chill, accompanied with abdominal pain, which was then attributed to his having eaten heartily of pork and beans; this pain was relieved by a sinapism applied to the abdomen, and the administration of tinct. opii gtt. xxx. The paroxysmal fever left him at six o'clock P. M., at which time he was again comfortable.

About 4 o'clock A. M. next morning, I was awakened by the hospital steward, who stated that the patient was taken suddenly very ill. I found him cold and almost pulseless, and tormented with constant retching and hiccup. Sinapisms were applied over the stomach, and to the legs and feet, and cyanide of potassium was given, but immediately rejected by the stomach; some warm wine was also given, but with similar results, ℞.—Ammon. carb. gr. v; spts. ammon. aromat. gtt. xx; spts. vini gallici, aquæ, āā f℥j. M. To be repeated every ten minutes. The patient was entirely sensible, but suffering much from the idea of approaching death. The vomiting ceased for a time, but despite every effort he continued to sink, and died at 5.40 A. M.

Post-mortem, six hours after death.—Rigor mortis well marked. Profuse hypostatic congestion. Abdomen slightly tumid and tympanitic. Suspecting purulent absorption, or phlebitic inflammation of the portal or hepatic veins, I had the abdominal cavity first laid open. The first incision therein gave free exit to a quantity of highly fetid gas, followed by serum profusely mixed with fleaks of coagulable lymph. About two pints of this fluid were found within the peritoneal sac. The great omentum was engorged with dark blood, which gave it the appearance of incipient gangrene. The peritoneal coat of the liver and intestines presented a brownish-black colour, smeared over with purulent lymph, and softened very much, bearing a close resemblance to those parts when disorganized by protracted chronic dysenteric inflammation. The substance of the liver presented a healthy texture.

Owing to the almost fatal consequence that resulted to myself from inoculation with some of the matter from his wound on the day I performed the operation, I was deterred from having the examination carried any further. The results were so serious that, taken in connection with the foregoing case, I deem them sufficiently interesting to record.

On the morning of the 15th of March, I accidentally pricked the end of my left index finger, on its palmar surface, with a small point of oak splinter, not larger than the point of a common pin, which scratched away a small portion of the epidermis. It caused no more molestation than a momentary pang, so that I overlooked its presence the next day when operating on the patient, Nelson. During the ensuing evening, while making an anatomical preparation of the amputated limb, I perceived a peculiar numbness in the injured finger, while manipulating therewith, but, attributing it to fatigue, I gave it little or no attention then, and retired to rest at a late hour. I awoke some hours afterwards, suffering very much with a throbbing pain

in it. In the morning, I found the whole finger red and swollen, and the lymphatics of the forearm and arm in the same condition up to the axilla. I applied cold water to the limb for some time, but finding the pain and inflammation increasing, I began to suspect that I was poisoned by absorption of some of the putrid matter while performing the operation before cited. By evening, I was bordering on wild delirium caused by the agonizing pain which I was enduring. The rapidity and virulence with which the poison acted, resembled very much that of the rattlesnake; so much so, that I feared it would affect me fatally when the rapidly progressing inflammation should reach the body. I immediately commenced taking large doses of bromine in combination with iodide of potassium, frequently repeated. Rest or sleep was impossible, and opiates made my sufferings worse, by producing disagreeable hallucinatory ideas. Next day, the finger was tensely swollen, infiltrated with fluid, with three or four large phlyctæna, giving it the appearance of gangrenous inflammation. The irritability was such that I could not touch it without producing intense suffering, so that it was impossible for myself to incise the parts requiring it. Cold water alone afforded me any relief. A violent attack of intermittent fever, with a protracted cold stage increased my wretchedness. I took of blue mass and quinia each ten grains. The following day, my friend, Dr. C. B. Hughs, arrived from Tuczon, and freely incised the finger, which lessened the pain somewhat. I now commenced taking chlorate of potassa in connection with the bromine. A repetition of the intermittent paroxysm produced so much prostration, that it was deemed proper to allow me some stimuli, consisting of wine, spirits of ammonia, and beef tea. The suffering from loss of sleep was intense and unrelieved from the commencement of the malady until the evening of the fifth day. A system of constant irrigation with cold chlorinated water induced the most fortunate results, by checking the tendency to suppuration, and I had the gratifying pleasure to see the inflammatory process reduced down to the finger alone on the sixth day. There the results were severe, ending in sphacelus, which destroyed much muscular tissue and the first metacarpal phalanx. At this date, forty-six days subsequent to the injury, a slough, including the tendinous expansion around the end of the necrosed bone, has not separated. Partial anchylosis of the second joint has resulted, and it is probable that the condition of the first phalanx is irremediable by any other than surgical means.

FORT BUCHANAN, ARIZONA, May, 1861.

ART. III.—*Experimental Researches on Points connected with the Action of the Heart and with Respiration.* By AUSTIN FLINT, Jr., M. D., Prof. of Physiology and Microscopic Anatomy in the Bellevue Hospital Medical College, New York. (With three wood-cuts.)

IT is not intended in this paper to take up all points connected with either of the functions which will come under consideration. This, of course, would be inconsistent with its scope; for many are so demonstrable, and now so well established, that their consideration here would be a mere recapitulation of facts well known, and universally admitted. It is rather the object of the writer to present some original experiments, by which he hopes to elucidate points which are yet the subject of dispute among physiologists; and which, in his opinion, cannot be settled by argument alone, but are capable of being brought under direct observation, and if established, can be made subjects of actual demonstration. Some functions cannot, as yet, be disclosed to our senses in their natural operation; but there are others, among which are the questions herein to be considered, which require only correct description to serve as immutable facts, from which each inquirer may make his own deductions. For the understanding of those processes which can easily be described, and about which there can be no mistake, nothing is usually necessary but simple observation; but there are others, more delicate and obscure, which different experimenters see in different ways. In investigations into their phenomena we should strive to perfect our modes of observation, to devise means by which all confusing circumstances may be removed, to invent instruments which will make them more prominent, so that any observer, willing to take the trouble to look for himself, can see and interpret them in but one way. This is no less a desideratum in physiological than in pathological investigations; and we must seek means of physical exploration which will be to the physiologist what the stethoscope, the speculum, the ophthalmoscope, and our numerous modern exploring instruments are to the pathologist. Obstetricians might differ with regard to conditions of the os uteri, exploring only by the touch, when the speculum, exposing the parts to the eye, would leave no room for discussion: auscultators, listening with the naked ear, through the clothing of a patient, would dispute about sounds heard within the thorax, when they would agree if a stethoscope were applied to the naked chest. Thus, perfected apparatus enables the chemist and physiologist to demonstrate facts which would be obscure with less certain means of investigation. Many points in the physiology of the heart and of respiration are yet undecided; and it is by removing some sources of self-deception in the simple observation of phenomena, and by multiplying

demonstrable facts, the only true bases of general deductions, that I have endeavoured to go a step beyond what is already known and established.

The questions which I shall take up in this essay are, with reference to the heart:—

First: *Does the organ shorten or elongate during its ventricular systole or contraction?*

Second: *How far can we determine the cause of its regular and periodic action?*

Third: *What are some of the causes of arrest of the action of the heart?*

Fourth: *What is the mechanism of some of the nervous influences over the action of the heart?*

Fifth: *What is the mechanism of the action of the valves which guard the orifices of the heart?*

And, in regard to respiration: *What is the cause, and where is the seat of the impression, or "besoin de respirer," which is conveyed to the respiratory centre, and which excites movements in the various muscles of respiration?*

These have either never been fully understood and described by physiologists, or are now explained in a contradictory manner in the various systematic physiological works.

It was with the hope of contributing something to the farther elucidation of these obscure and disputed points that the experiments which form the basis of this paper have been undertaken.

Changes in Consistence, Position, and Form of the Heart, during its Action.—With regard to the physiological action of the heart, there is manifestly but one correct mode of study, and that is the one which led the immortal Harvey, one of the most conscientious observers science has ever known, to make his great discovery of the circulation of the blood. That is, to expose the heart in living animals during its action, and observe its movements. This may be done in various animals, and in various ways. It is easy to observe the heart in action in the cold-blooded animals, by simply removing the anterior walls of the thorax; as its contractions will continue for a long time after such an operation, and even after the organ has been entirely separated from the body. Such observations give us a great deal of information, but are made more valuable when we compare them with phenomena observed in warm-blooded animals, in which the heart resembles the corresponding organ in man. In operating upon the heart of these animals, such as dogs, cats, sheep, horses, etc., it is necessary to keep up artificial respiration, as this function cannot, of course, be performed by the animal after the thorax has been opened. Here it is convenient, as well as humane, to abolish sensibility by some means which will not interfere with the heart's action. This may be done by crushing the

medulla oblongata, in such a way as to avoid hemorrhage, as is done by Erichsen and Pavy, of London; by stunning the animal with a blow upon the head, as was done by Drs. Pennock and Moore, of our own country; by decapitation, and ligature of the vessels of the neck, as was done by Legallois; by inoculation of woorara, or by the administration of ether or chloroform, which are the most convenient modes, and now most commonly employed by physiologists. The experiments which I have made have been performed upon animals rendered insensible by woorara or ether; and I have been accustomed to operate in the following way.

The animal (generally a good sized dog) is first poisoned with woorara, by injecting about a grain of this substance into the subcutaneous areolar tissue, or completely etherized. If poisoned with woorara, its effects are watched, and in from ten to thirty minutes the dog comes under its influence; more readily, if he be made to move about. If ether be used, he is rendered insensible in the ordinary way. The trachea is then opened, and the nozzle of an ordinary bellows introduced for the purpose of keeping up artificial respiration. An incision is then made in the median line from the top of the sternum to a point somewhat below the ensiform cartilage, through the skin, fascia, and fat. The next step is to cut through the superficial muscles the whole length of the sternum on each side, about an inch from the median line, down to the costal cartilages, and we may then tear away the muscles from the chest, exposing the ribs, without much hemorrhage. The next step, after having exposed the chest in this manner, is to saw through the sternum in the median line, opening into the thoracic cavity. We can then hold open the chest by sticks, or, what is more convenient, cut across the ribs on each side with a pair of strong cutting pliers, fold back the anterior walls of the thorax, and retain them in that position by a strong ligature passed under the back of the animal, and firmly tied. In this way we expose the lungs, which are regularly inflated by the bellows, and between them, the heart inclosed in its pericardium. The pericardium may then be removed by slitting it up, and cutting it away from its attachments at the base of the heart, and we can observe this organ in the natural performance of its functions.

When the heart of a dog is exposed in this way, one of the most constant effects is an increase in the rapidity of its contractions. The pulse of a dog is always irregular in a state of health, varying from 100 to 120 beats in the minute. When the heart is exposed, its pulsations become more frequent, sometimes numbering 200 to 250.

The phenomena which we observe in connection with the contraction of the ventricles, are:—

1st. *Hardening.*—This we would expect from the structure of the heart; for we know that it is a phenomenon constantly attending muscular contraction. It was described by Harvey, who proved that it took place during contraction of the ventricles, by introducing a small canula through

the walls of the left ventricle, applying the hand to the heart, and noticing that the hardening took place when a jet of blood was forced through the canula. I believe that all physiological authors agree on this point, though Dr. Wood, late of the University of Pennsylvania, does, or, at least, did hold the opinion that the heart hardened during the diastole. I have repeatedly verified the fact that the heart hardens during the systole, by repeating the experiment of Harvey. One who examines the heart in action cannot be mistaken in regard to this point.

2d. *Tilting upwards of the point of the Heart, and Locomotion of the Apex from left to right.*—About this point there is no difference of opinion. It can easily be observed in vivisections, and this movement would be expected from the spiral and *oblique* course of the superficial fibres of the heart from right to left, arising at the base, and inserted, as it were, into the apex, which is free.

3d. *Twisting from left to right.*—This can be observed by examining the apex of the heart. It is universally admitted by physiologists, and is explained by the spiral course of the fibres from right to left. This phenomenon, like the preceding, I have repeatedly observed.

4th. *Elongation of the Ventricles.*—This, which I hope to convince the reader to be a fact, is denied by all modern French, English, and German physiologists, who seem all to agree that Harvey was wrong in this part of his description of the action of the heart. Harvey, Vesalius, Riolan, Fontana, Barelli, Winslow, and Queye, contended for the elongation of the organ during its systole; but this view was combated by Steno, Lancisi, Bassuel, and, finally, by the great Haller, who has exercised more influence over physiological literature, perhaps, than any man before or since his time. It seems to me that the prevalence of the opinion, at the present day, that the heart shortens during systole can be attributed, in great measure, to the weight of the opinion of Haller. I am fortunate in having an opportunity of referring to an edition of his original works, published in Latin in 1757, and could not but be struck with its similarity in views, in arguments, and sometimes even in actual mode of expression, when treating of the change in the length of the heart during the systole, with the works on physiology which are now used as text-books, especially those by French authors. Thus it is that scientific opinions are left by such men as legacies to future generations; and are copied from one work to another, true or false, as the case may be. For, in making books, authors have but little time to investigate for themselves, even in points so easily observed as the one under consideration.

Haller bases his views upon his own experiments—upon a case of ectropy of the heart ("Denique in puero, cui cor extra pectus propendebat, *cor in diastole longius, et in systole brevius factum est, perinde ut in bestiis videmus*"[1]) and upon an argument of Bassuel. This last argument against

[1] Elementa Physiologiæ, tom. i. p. 392.

the elongation of the heart is employed by many physiologists of the present day; though it need hardly be said that all arguments are powerless against a fact which can be satisfactorily demonstrated. Haller, after stating the views and arguments of Vesalius, Riolan, and others, says:—

"*Varia nuperrimi scriptores reposuerunt. Et quidem Cl. Bassuel ad argumentum a valvulis venosis repetitum respondit, earum fabricam contra adversarios facere. Si enim in systole cordis mucro a basi recideret, tunc certe sequeretur, ut adtractis ad apicem funiculis, valvulæ in cordis caveam deductæ ostium aperirent, sanguinique venoso eam viam referarent, quam utique clausam esse oportet, dum cor contrahitur Mihi vero videtur, valvulas quidem venosas eo tempore a sanguine versus aures repulso extrorsum, inque aurium cavitates cessuras, nisi a musculis suis papillaribus, eo ipso tempore se decurtantibus, retinerentur, inque ventriculum reducerentur.*

Aliud experimentum addidit Cl. Bassuel; cor nempe aqua replevit, viditque, dum brevius fiebat, aquam expelli."[1]

I have exposed thus fully the views of Haller on this subject, because of the commanding influence he so long exercised in the physiological world, and especially because, on this point, late authors seem to have followed him so closely. In addition, it may not be uninteresting to cite a few of the authorities which favour the shortening of the heart during its systole, to show how generally received is the view which I expect to disprove.

TODD, in the article on the heart in the *Cyclopedia of Anatomy and Physiology*, speaking of the organ during its systole, says: "In all warm-blooded animals, at least, it becomes shortened."

CARPENTER, in the *Principles of Human Physiology*, London, 1855, page 226, says:—

"During their contraction, the form of the ventricles undergoes a very marked change, *the apex of the heart being drawn up towards its base*, and its whole shape becoming much more globular."

KIRKES, *Handbook of Physiology*, in speaking of the action of the ventricles, says:—

"They contract much more slowly than the auricles, and simultaneously in every part, the whole wall of each ventricle being drawn up uniformly towards the origin of the artery at its base, diminishing the cavity in every diameter, *but especially in length, so that the heart assumes a shorter and more globular form than it had in the relaxed and distended state of the ventricles.*"

Among the French authors we find in BÉCLARD, *Traité de Physiologie*, Paris, 1856:—

"Le Raccourissement général de l'organe, au moment de la contraction des oreillettes, est assez limité. *Son plus grand raccourssement coincide avec la contraction des ventricles*, qui l'emporte par dimensions les oreillettes" * * * "chez quelques animaux, le raccourissement suivant la verticale est moins prononcé que le raccourissement sur l'horizontale, ce qui a fait penser faussement à quelques observateurs que le cœur s'allonge pendant la systole ventriculaire."

RICHERAND, *Eléments de Physiologie*, tome i. p. 478, says:—

"D'après cela, il est evident que le cœur se raccourcit," meaning during the systole.

[1] Elementa Physiologiæ, tome i. p. 391.

BÉRAUD, *Eléments de Physiologie*, Revus par Ch. Robin, tome ii. page 277, says, speaking of the systole:—

"Le sommet des ventricles se rapproche de la base et du sommet, il suit de lá que la cœur se raccourcit."

MAJENDIE, *Précis Elementaire de Physiologie*, tome ii. p. 395, says:—

"*Les partisans de l'allongement ne persistent plus;* mais il restait à démontrer comment, les ventricules se raccourissant, le cœur se porte en avant."

BÉRARD, *Cours de Physiologie*, Paris, 1851, tome iii. page 603, speaking of the systole, says:—

"Le sommet des ventricles se rapproche de la base, et la base du sommet; *il suit de lá que le cœur se raccourcit.*"

M. H. MILNE EDWARDS, in his *Leçons sur La Physiologie et L'Anatomie comparée de l'Homme et des Animaux*, tome iv. page 19, now in course of publication, in speaking of the systole of the heart, says:—

"En effet, il devient presque circulaire à sa base; la portion voisine de la région ventriculaire se bombe d'une manière assez régulière, *et la portion inférieure qui avoisine la pointe rétrècit et se raccourcit.*"

Finally, in the *Traité de Physiologie considérée comme science d'observation*, par C. F. BURDACH, Professor à l'Université de Konigsberg, translated into the French by Jourdan, we have the opinion of the German physiologists; for Burdach was assisted in the preparation of this work by Baer, Mayen, Meyer, J. Müller, Rathke, Valentin, and Wagner: tome vi. page 234, he says:—

"La contraction, *ou systole*, s'opère avec la rapidité de l'eclair. Le cœur se reserre sur lui-même; il devient plus ferme et plus dur; *il se raccourcit*, c'est à-dire que sa base et son sommet se recourbe un peu."

We thus see what a weight of authority there is in favour of the shortening of the heart during the systole; all of the English, French, and German authors holding this opinion, and all of them denying the description of Harvey, who states that the heart elongates during contraction; "that it is everywhere contracted, but more especially towards the sides, so that it looks narrower, *relatively longer*, more drawn together."[1] It is only in this country that this opinion has been controverted; and though experiments have been made in England, with reference to this point,[2] they confirmed the prevalent view, and American experiments thus far have stood alone.

In November, 1839, Drs. PENNOCK and MOORE made a number of experiments upon the hearts of rams and young calves, in order to settle disputed points in the change of the form of the heart during its action, and

[1] Harvey's Works, published by the Sydenham Society, page 21.

[2] Experiments on the Motions and Sounds of the Heart, by the London Committee of the British Association for 1838–39, and 1839–40. Experiments for 1839–40. Hope on the Heart, Amer. ed. 1846, page 65.

the mechanism of the production of the heart sounds. These were published in the *Philadelphia Medical Examiner*, No. 44, and also in the American edition of *Hope on the Heart*, 1846, page 59. It is not my object minutely to detail these experiments; I will simply state that the animals operated on were stunned by a blow on the head, a bellows introduced into the trachea, by means of which artificial respiration was kept up, the chest opened, and the movements of the heart observed. With reference to the form and length of the heart during its systole, in all of these experiments, Drs. Pennock and Moore found *that the heart elongated.* Its elongation was measured with an ordinary shoemaker's rule, and found in one experiment (an ewe, one year old), to be one quarter of an inch.

There are many sources of difficulty in examining a phenomenon, apparently so simple as that of elongation or shortening during the systole of the heart. In the first place, in the warm-blooded animals, as the dog, the heart's action is so rapid that it is difficult at first to determine even which is the systole and which the diastole. Then in examining the heart when the lungs are being alternately filled and emptied, partly covering the organ at each expansion, its apex only is seen, and that appears to retract when the heart contracts. In order to demonstrate the period of contraction in systole of the ventricles, I have employed the proceeding of Harvey, of pushing a small silver tube through the walls of the heart into the left ventricle, withdrawing the stylet; at each systole, a small jet of blood is forced through the tube, which enables one to determine at a glance when it takes place. In order to determine the period of elongation and shortening of the heart, I have devised an apparatus by means of which this phenomenon is exaggerated, and can be made apparent to any number of witnesses; so that by introducing the canula, the period of ventricular contraction may be determined at the same time with the alterations in the length of the heart, and their relation to each other incontestably established.

This apparatus, which I have called the *mekeoscope*,[1] is simple in its construction, and easily applied to the exposed heart of a living animal. The one which I have used in my experiments is represented by the accompanying figures. It consists of a piece of light wood (*A*) about three inches square, and from $\frac{1}{8}$th to $\frac{1}{6}$th of an inch in thickness, slightly hollowed out at one edge. Near one end are fixed two uprights (*B*) $2\frac{1}{2}$ inches in height, and between them a thin piece of wood (*C*) is so arranged upon a cross-bar (*F*), which moves on pivots (*D*), that any motion at the lower extremity is indicated by the long arm above. This is kept in place by a slender elastic thread (*E*) attached to the uprights behind, and passing over it in front. Any movement at the lower end of the indicator may be still farther exaggerated by attaching a feather to its upper extremity.

[1] From μῆκος εος, length, and σκοπειν, to observe.

In using this apparatus, the heart of the animal is placed upon it so that its apex comes in contact with the lower end of the indicator. A

Fig. 1.

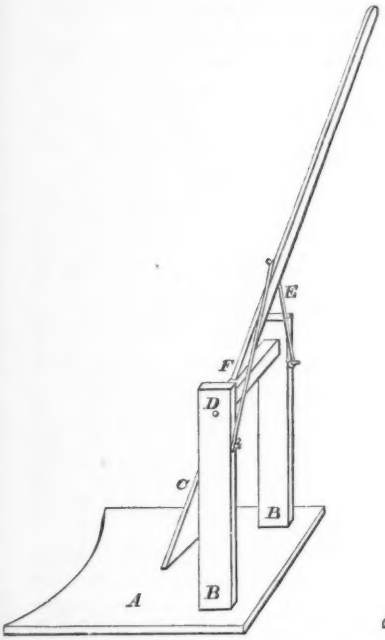
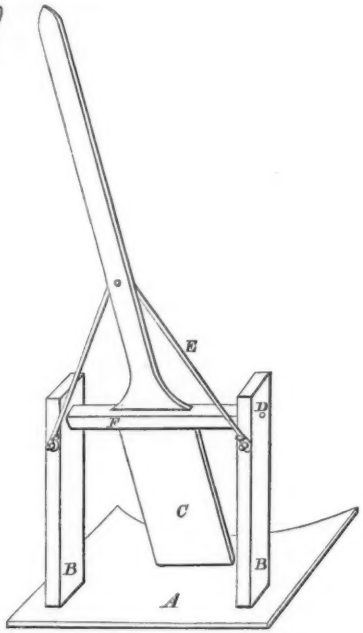


Fig. 2.



small tube may now be introduced into the left ventricle through its walls, and the instrument will indicate elongation of the heart at every systole which is marked by the jet of blood propelled through the tube.

The lower end of the indicator is kept in contact with the apex of the heart by means of the elastic thread; and as it might be said that pressure upon the apex during its diastole was the cause of the apparent shortening of the heart at that time, I removed it, and attached the indicator to the heart by means of a thread passed through the apex. The instrument did not follow the movements of the heart so completely as before, but elongation during systole and shortening during the diastole were still marked.[1]

[1] The experiments which I have made upon the heart of the dog are numerous, and their results have been nearly identical. I shall, therefore, detail such as have reference to the various questions which are to be taken up in this paper, and where succeeding observations have merely confirmed them, simply state the fact.

In reasoning from the action of the heart in the lower animals to the corresponding function in man, we must take into consideration similarity of structure and arrangement of the organ, and also take care that the ordinary conditions of life should approximate as nearly as possible to those in the human subject. For this reason our most valuable experiments are on the warm-blooded animals; and the phenomena which we find here are not always verified when we descend in the scale. I have not touched upon the change in form of the heart in the cold-blooded animals in the body of this paper; for, though such investigations are interesting in themselves, they do not teach us much with regard to human physiology, which is, of course, the great end of all our physiological inquiries. I may here state, however, that in the turtle the heart shortens during systole. This is due to the thinness of the ventricle and the great size of its cavity compared with the warm-blooded animals. The heart of the frog, also, shortens slightly during systole, and I have been able to measure the actual extent of shortening by a pair of ordinary dividers. An American observer[1] has described an experiment, which I have often repeated, to prove the elongation of the frog's heart during contraction, which consists in holding the heart by the base between the thumb and finger, with the apex upward, and irritating it with the point of a needle. At each irritation when the apex will be elevated, giving an appearance of elongation. This appearance is not deceptive; the heart actually elongates, but the position in which it is held, the ventricle being empty, causes its flaccid walls during relaxation to collapse, shortening the heart more than is natural. The same experiment I have repeated with the heart of the turtle; but, in altering the position of the heart and allowing the apex to hang downward, the heart will be found to shorten during the systole, the dependent apex being drawn up by the muscular contraction. These experiments prove nothing one way or the other. It is better, in all experiments of this description, to observe the heart *in situ*, while its cavities are filled with blood. In observations upon the irritability and various properties of the anatomical elements of the heart, we can study phenomena in cold-blooded animals with advantage; for here the properties are the same, modified only by the vital condition of the animal, which, by diminishing the intensity of their manifestations, render their study more simple.

The following experiments were made with reference to the change in the length of the heart during the systole or contraction:—

Experiment I. Jan. 28, 1861.—A good-sized dog was poisoned with woorara, artificial respiration was kept up, and the heart, which was beating strongly and naturally, exposed in the usual way. Upon holding the base of the heart between the index and middle fingers, the thumb, placed upon the apex, was sensibly raised at each systole, which was marked by the hardening of the ventricles. The mekeoscope was now applied, and indicated elongation with every

[1] Dalton's Treatise on Human Physiology, 2d edition, page 258.

systole, or contraction of the ventricles. The systole of the heart was also marked by slight corrugation of the surface of the ventricles, by a tilting movement of the apex upwards and from left to right, and by a twisting movement of the heart on its axis, from left to right.

Expt. II. Feb. 1, 1861.—A medium-sized dog was etherized, and the heart exposed in the usual way. The facts which are recorded in Expt. I. were demonstrated upon this animal. The mekeoscope was applied to the heart, which was acting normally, and indicated elongation during the systole. This point was verified by several medical gentlemen. The upper extremity of the indicator moved from one-half an inch to an inch with every beat of the heart. It was determined that the heart elongated during the systole, by introducing a small silver canula into the left ventricle, and noticing that the indicator showed elongation every time a jet of blood was forced through the canula.

Expt. III. Feb. 8, 1861.—A large dog was poisoned with woorara, and the heart exposed in the usual way. The heart was pulsating well; the mekeoscope was applied, and elongation during the systole was demonstrated. The various points recorded in Expt. I. were confirmed in this animal.

Expt. IV. Feb. 15, 1861.—A medium-sized dog was etherized, and the heart exposed in the usual way. The mekeoscope was applied, and the points recorded in the preceding experiments were confirmed.

Expt. V. Feb. 19, 1861.—A good-sized dog was etherized, and the heart exposed in the usual way. The points recorded in the preceding experiments were confirmed in this.

Conclusions.—From the five observations here reported, which I have repeatedly confirmed in unrecorded experiments, there is one legitimate conclusion, viz: *that the heart of the dog elongates during the systole or contraction of the ventricles.* In reasoning from the inferior animals to the human subject, taking into consideration the anatomical characters, we find that the heart of a dog has the same essential anatomy as that of man. But instances are on record where the heart has been exposed to observation even in the human subject. Harvey states, in his report of the remarkable case of the son of the Viscount Montgomery:—

"We also particularly observed the movements of the heart, viz., that in the diastole it was retracted and withdrawn, while in the systole it emerged and protruded; and the systole of the heart took place at the moment the diastolic impulse in the wrist was perceived: to conclude, the heart struck the walls of the chest, and became prominent at the time it bounded upwards and underwent contraction of itself."[1]

Haller states that, in the case of ectropia of the heart which he had an opportunity of observing, the heart shortened during its systole, as it did in animals that he examined. Both of these eminent physiologists had the opportunity of examining the action of the heart in the human subject, and both verified their previous observations on animals, though the results were contradictory.

It seems, then, a legitimate conclusion that, in man, as in the animals examined, *the heart elongates during its systole.*

Having come to this conclusion from actual observation, the next step is to endeavour to account for it by the anatomical arrangement of the muscular fibres of the heart. This can easily be done; for, if a heart be boiled

[1] Harvey's Works, published by the Sydenham Soc., page 384.

so as to dissolve the areolar tissue which holds together its muscular bundles, the fibres can easily be separated and traced, and we have, on the outside, a layer of fibres, common to both ventricles, taking a spiral course from left to right, from the base to the apex; and if we remove these superficial fibres, we will find beneath them a mass of circular fibres, enveloping separately the right and left ventricles. The action of these circular fibres in shortening is, of course, to increase in diameter; and this increase in diameter, from the arrangement of the fibres, would produce elongation of the body of the heart.

The powerful action of these deep circular fibres of the heart is also shown by a phenomenon noticed during contraction, namely, the production of rugæ on the surface of the ventricles. This appearance is not mentioned by Harvey, but is noticed by Haller in the work I have before quoted, vol. i. page 389.

"*Quando cor quietum, aut relaxatum, a stimulo quocunque in motem cietur, tunc apparent in externa cullis superficiæ rugæ, in quas fibræ contractæ crispantur, undulatæ, in rana et anguilla evidentur transversæ, neque in cane, fele, aut aliis calidi sanguinis animalibus obscuræ.*"

This may be because the superficial fibres, being exposed to the air, are more irregularly and less powerfully contracted than the deep, or it may be a phenomenon which always takes place. At all events, it indicates that the powerful contraction of the deep circular fibres throws the superficial ones into slight longitudinal folds from the greater efficiency of their action; and that the superficial fibres, which from their arrangement might tend to shorten the heart, do not compress, in their contraction, the deep fibres, but that the latter are the more powerful agents in the systole. Thus what is demonstrated by observation of the action of the heart in the living animal, may easily be accounted for by the anatomical arrangement of its muscular fibres.

In regard to the hypothesis of Bassuel, which is so often quoted, that elongation of the heart, by putting the cordæ tendinæ on the stretch, would prevent the closure of the auriculo-ventricular valves, and is, therefore, impossible, I have nothing to say. It seems to me sufficient to have demonstrated on the living animal the elongation; and I have this simple *fact* to oppose to any hypothetical objection.

Cause of the Rhythmical Contraction of the Heart.—The cause of the regular and intermittent contraction of the heart is obscure; and the experiments which I have made on this subject, though far from being as satisfactory as the preceding, still, I conceive, define the extent of our knowledge, and bring to light some laws which regulate the action of this organ. It was first supposed that the blood, circulating through the heart, was the cause of its rhythmical contraction; but the heart will continue to beat regularly after it has been emptied of blood, and removed, indeed, from the

body. The atmosphere was then supposed to supply the place of the blood as a stimulus; but the heart of a frog has been placed under the receiver of an air-pump, and still it continued to pulsate. Without going farther into the opinions now entertained by physiological writers, it is sufficient to state that we are not yet fully acquainted with the real cause of the rhythmical contraction of the heart; and the following experiments were instituted with the hope of throwing some light upon this obscure subject.

Expt. VI.[1] Nov. 14, 1860.—An alligator, six feet in length, was poisoned with woorara, the thoracic cavity opened, and the heart exposed. Some experiments were then made upon this organ which will be detailed in another place; but twenty-four hours after the operation, the heart, which had been left in situ, was found beating regularly and with considerable force.

Expt. VII. Jan. 28, 1861.—An alligator of the same size as the one used in Experiment VI., *i. e.*, six feet in length by measurement, was poisoned with woorara, the chest opened, and observations made upon the heart. The heart was left in situ, and in twenty-four hours after death was found pulsating vigorously and regularly. The auricles were then galvanized by an ordinary magneto-electric apparatus, during the intervals between the movements of the organ; they immediately contracted, and their contraction was immediately followed by contraction of the ventricles. Upon applying the stimulus to the ventricles they contracted, contraction of the auricles following immediately. These phenomena were repeatedly verified in the presence of two assistants; the same result followed irritation with the point of a scalpel. The heart was then removed from the body, and emptied of blood. When placed upon the table, it pulsated quite rapidly (about ten times per minute, instead of four or five), but its contractions were feeble. Upon galvanizing the ventricles, they contracted powerfully, followed by feeble contractions of the auricles. Upon galvanizing the auricles, they generally contracted feebly, and sometimes no movement was excited; but the ventricles contracted invariably.

The aortæ (there are two in the alligator) were then tied, and the heart filled with blood (which was prevented from coagulating by the addition of a little solution of carbonate of soda) by injecting it through the right auricle, and confining it by a ligature. The heart then commenced to contract regularly and forcibly. The auricles contracted first, and then the ventricles, making about four pulsations per minute. The contractions were now powerful and regular, contrasting strongly with the rapid and feeble action before the organ had been filled with blood. The heart was evidently over-distended, but was relieved by dividing the coronary artery, and allowing some of the blood to escape, until it was reduced to about its normal condition of fulness. Galvanization of the auricles excited contraction, followed by contraction of the ventricles, and the same stimulus applied to the ventricles excited contraction, followed by contraction of the auricles, in about the same manner as when these experiments were made upon the organ before its removal from the body. This, also, was repeatedly verified. The heart was then emptied of blood, and placed upon a clean plate. The contractions became such as were noted immediately after the heart had been removed from the animal, and before it had been filled artificially with blood.

The heart was then filled with water. The contractions were not so powerful and regular as when it had been filled with blood, and were limited chiefly to the ventricles. They were also much more rapid. It was impossible to establish the contraction of the auricles on galvanization, followed immediately by contraction of the ventricles, and the reverse as when the heart was filled with blood. The ventricles, still filled with water confined in their cavity, were then firmly compressed by the hand, so as to subject the muscular fibres to powerful compression. From that time the heart entirely ceased its contractions, and

[1] For the anatomy of the heart of the alligator, see Appendix.

became hard, like a muscle in a state of cadaveric rigidity. The experiment was then terminated *twenty-eight* hours after the death of the animal; the heart was then beating until its pulsations were arrested in the manner described.[1]

Expt. VIII. *On Turtles.*—The hearts of turtles were exposed and removed from the body while pulsating. Galvanism applied to the auricles induced contraction, followed by contraction of the ventricle; and a stimulus applied to the ventricle induced a contraction followed by contraction of the auricles. This took place whether the irritation were galvanic, or mechanical, like the point of a needle, and indifferently whether the heart were removed from the animal or left *in situ*.

These observations were repeatedly verified upon the same turtle, and in a number of subsequent experiments.

Expt. IX. March 11, 1861.—The heart of a turtle was removed, and the ventricle separated from the auricles. The auricles contracted spontaneously and regularly for twenty minutes, the time during which their movements were observed, and the ventricle contracted irregularly and at intervals of two minutes or more. The ventricle always contracted when irritated with the point of a needle.

(In this experiment I was not certain that the ventricle contracted without the intervention of a stimulus, for it was exposed to currents of air, jars of the table, etc., which might be capable of inducing contractions.)

Expt. X. Mar. 13, 1861.—The heart of a turtle was removed from the body while it was beating regularly, and the ventricle separated from the auricles as in the preceding experiment. Both auricles and ventricle were then placed under a bell glass and carefully observed for an hour and thirty minutes.

When placed under the bell glass, the auricles contracted regularly from 12 to 16 times per minute. No contraction of the ventricle.

Five minutes after. Auricles the same and the ventricle contracted once.

Seven minutes after. Auricles contracting regularly 16 per minute, ventricle contracted. (The apparatus was shifted from one table to another, which might have been the cause of the ventricular contraction.)

Ten minutes. Auricles the same, and ventricle contracted.

Eleven minutes. Ventricle contracted.

Twelve minutes. Ventricle contracting regularly five times in two minutes.

Twenty-two minutes. Ventricle contracting regularly seven times in two minutes; auricles contracting twenty-two times per minute.

Thirty-two minutes. Ventricle contracting four times per minute; contractions of auricles rapid but irregular.

Forty-two minutes. 1¾ minutes between the contractions of the ventricle.

One hour and thirty minutes. Auricles contracting eight times per minute; two minutes between the contractions of the ventricle.

Expt. XI. Mar. 13, 1861.—The heart of a turtle was removed and placed under a bell glass.

Thirty minutes after. It pulsated twelve times per minute, contraction of the auricles always preceding that of the ventricle.

Sixty minutes. The heart pulsated six times per minute, the auricles contracting first.

(In making these experiments, it was found that the operation of removing and dividing the heart produced a shock which interfered at first with its action. The heart recovers from it, however, in about thirty minutes.)

Expt. XII. Feb. 15, 1861.—A medium-sized dog was etherized, and his heart exposed in the usual way, artificial respiration being kept up. While respiration was being actively performed by means of bellows, and while the heart was pulsating vigorously, the organ was suddenly removed from the body by a single sweep of the knife. It was immediately placed upon the table, and contracted so vigorously that *it bounded up at every pulsation like an India-rubber ball.* This remarkable phenomenon lasted for a few seconds only, but the heart pulsated regularly for two minutes. A powerful shock was then passed through

[1] These observations were made twenty-four hours after the death of the animal.

it by means of the magneto-electric apparatus with the effect of immediately arresting all regular pulsations, but was followed by a general, irregular, vermicular action of the fibres. This continued for thirty minutes. At first, irregular contractions could be excited by feeble currents of galvanism, but after thirty minutes this became impossible. When the vermicular action of the muscular fibres had ceased, no contraction could be excited by galvanic or mechanical stimulus.

Expt. XIII. Mar. 13, 1861.—A turtle was poisoned with a variety of woorara, which arrests or depresses the action of the heart, by injecting about a grain of it, in solution, under the skin. In thirty minutes the animal was dead and the heart was exposed, which was beating feebly and slowly. On applying galvanism to the exposed muscles their irritability was found, by actual comparison with the exposed muscles of turtles which had not been poisoned, to be very much diminished.

Expt. XIV. Mar. 13, 1861.—The preceding experiment was repeated upon another turtle. When all signs of life had disappeared, the heart was exposed and found beating feebly. Muscular irritability was much diminished.

It will be seen by these few experiments that it is difficult, if not impossible, in the present state of our knowledge, and with such data alone, to say why the heart contracts in the manner which is characteristic of it. If the cause reside in the nervous system, it must be in nervous centres existing in the very substance of the organ. In short, the contraction of the heart is dependent either upon nerves in its substance, or, upon an inherent property peculiar to its muscular fibres. The nervous influence, if there be any, must come from the sympathetic or organic system, because an organ must remain connected with the cerebro-spinal centres in order that any influence should be derived from this system.

Dr. Robert Lee, of London, has demonstrated the existence of sympathetic ganglia in the substance of the heart; but it is impossible to say positively that an influence derived from these is the cause of its rhythmical contractions. The most that we can say on this subject is, that the muscular fibres which go to form the heart have an inherent property of contraction, so long as they are in a state of physical and chemical integrity; that this contraction, like that of all other muscles, is followed by a relaxation, but the fibres of the heart, after the short period of repose which is thus allowed them, contract again. I know that in this statement we are simply describing the phenomena of the heart's action, and confessing our ignorance as to its cause; but we are in the best position to acquire information upon any subject when we admit the real state of our knowledge, and do not attempt to explain what, with our resources, is incapable of explanation.

Having thus stated the sum of our actual knowledge of the cause of the heart's action, we are prepared to study some of the properties of the muscular fibres of this organ, and the laws by which their contractions are regulated.

1. The muscular fibres of the heart possess, in a remarkable degree, that property known as irritability. This is more marked in the auricles than in the ventricles. They contract readily upon the application of a stimulus

applied to the surface, and Virchow has demonstrated that the internal surface is much more irritable than the exterior.

2. It has been shown by some experiments instituted by Erichsen,[1] that the action of the heart is arrested in about thirty minutes, in the warm-blooded animals, by ligature of the coronary arteries, artificial respiration being continued; showing that the presence of a certain quantity of blood in the substance of the organ is necessary to the irritability of its muscular fibres.

3. Experiments here detailed, as well as those of others, demonstrate that the heart of the cold-blooded animals, especially the alligator, retains its irritability for a long time after death. In Expt. VII. the heart was beating vigorously twenty-eight hours after death, when it was artificially arrested. This was due in part to the action of woorara; for Bernard has lately shown that muscular irritability remains in frogs poisoned with this substance much longer than ordinary, and that the action of the heart is also prolonged.[2] This property renders the woorara a valuable agent in studying the movements of the heart.

4. The same experiments (on turtles and alligators) show that a stimulus, mechanical or galvanic, applied to one part of the heart, is propagated to the other; and tend to show that the stimulus which, in the natural action of the organ, excites the auricles to contraction, is propagated from them to the ventricles. These experiments are not new. The same thing has been noticed by Mr. Paget in the heart of the turtle, and was published by him in the *British and Foreign Medico-Chirurgical Review*, vol. xxi. page 550.

5. Experiments IX. and X. show that the irritability of the auricles and ventricles are separate and distinct; that the auricles possess this irritability to a much greater extent than the ventricles, as demonstrated by the distinct contractions of auricles and ventricles when separated from each other, and the much greater frequency of contractions of the auricles. That the contraction of the auricles acts as a stimulus to the ventricles; for when they are left together, as in Expt. XI., and the heart removed from the body, the auricles always contract first, and their contraction is invariably followed by contraction of the ventricles.

6. Experiment VII. shows that the heart of the alligator, if emptied of blood, does not contract regularly; but that its regular contractions are resumed if the blood be injected into, and confined in its cavities. Also

[1] These experiments were made by pithing the animal, keeping up artificial respiration, and opening the chest. It was found that the heart continued to beat under those circumstances, for from one to two hours, but was arrested in a short time if the coronary arteries were ligated. The mean of six experiments showed the duration of the heart's action, after ligation of these vessels, to be 23½ minutes. The experiments are to be found in the *Medical Gazette*, July 8, 1842.

[2] Bernard, "Substances Toxiques et Medicamenteuses," page 320 *et seq.*

that the propagation of stimulus from auricles to ventricles is not invariable in the heart emptied of blood, but that it may always be demonstrated in the heart filled with blood, naturally and artificially.

7. The same experiment shows that the heart filled with water will not act naturally after removal from the body as it will if blood be injected into its cavities, but more rapidly and less effectually; and, finally, that great compression seems to paralyze the muscular fibres instantly and cause them to take on cadaveric rigidity.

8. Experiment IX. shows that a powerful galvanic shock passed through the substance of the heart, in warm-blooded animals, immediately puts a stop to all regular pulsation.

9. Articles which abolish or diminish general muscular irritability like the sulphocyanide of potassium, have a corresponding effect upon the heart. This is a fact which is now well established.

Experiments XIII. and XIV. show that a certain kind of woorara which arrests the action of the heart, diminishes very much the general muscular irritability. This goes to prove the identity of the irritability of the heart and of the general muscular system.

Conclusions.—From the facts above enumerated, the following deductions can legitimately be made:—

The natural stimulus of the regular movements of the heart is the blood; and this stimulus cannot be adequately supplied by any other fluid of lesser density, like water. So that, in conditions in which the blood becomes watery, as in the reaction after copious bleeding or anæmia, we have feeble and rapid contractions of the heart; and in affections in which the blood becomes denser than in health, as in plethora, the heart contracts more slowly and with abnormal force.

In the natural action of the heart, this stimulus first affects the auricles, which are first distended with blood, and is propagated thence to the ventricles.

All irritability of the muscular fibres of the heart may be immediately arrested by forcible compression; and its property of regular contraction may be abolished by a powerful galvanic current.

A peculiarity about the muscular irritability of the heart is, that when the organ has ceased to contract spontaneously while in the chest, or after removal from the body, contractions cannot be excited by ordinary stimuli, such as irritation with the point of a needle or scalpel, or galvanism; while such irritation applied to any of the muscles will produce contractions. In an experiment which I made upon this point upon the heart of a dog, I found that the heart ceased beating in about ten minutes after the stoppage of respiration (the dog had been etherized and his heart exposed), and that after that time galvanism applied to the heart failed to produce contraction, though the sterno-mastoid and muscles of the chest, which had been exposed during the operation, contracted powerfully on the application of

the stimulus. This favours the idea that the muscles of the heart differ from the other striped muscles in possessing the inherent property of regular contraction, for they continue to contract till they have lost their irritability, and then cannot be excited to action artificially. This is only true when the heart is allowed to stop spontaneously, and the duration of its pulsations is not interfered with by placing it in a vacuum,—which, while it does not arrest, abridges the duration of the heart's action,—or by other means.[1]

The irritability, which in ordinary muscles is manifested by their contraction upon the application of a stimulus, and, in the case of the heart, by regular pulsations so long as the fibres retain their integrity, is really identical in the heart and general muscular system; it is greatest in the heart, and is much greater in the auricles than in the ventricles, as shown by experiments. In the heart this irritability becomes extinct before general muscular irritability is lost, for the regular contractions of the organ after death, or after removal from the body, wear it out; while the general muscular system, if unstimulated, is in a state of repose. It is also true that muscular, like nervous irritability, disappears soonest in parts where it is most intense, as it does in animals like the warm-blooded, the functions of which are most active. The ventricles seem to depend for their stimulus upon the contraction of the auricles; for when separated, as in Expt. X., the ventricles do not contract as frequently as the auricles, nor as frequently as when their connection with them is not severed. The ventricles possess then an independent irritability which is much less than that of the auricles.

The irritability of the heart is like the general muscular irritability in another respect. Most agents which paralyze the muscular system paralyze the heart; and Expts. XIII. and XIV. show that the peculiar variety of woorara which acts upon the heart, diminishes, to a great extent, the irritability of the general muscular system. On the contrary, the most common variety of woorara, which paralyzes the motor nerves and the sympathetic system, leaves the muscular irritability intact, and also the movements of the heart, which will continue for a long time after death, if respiration be artificially performed. This disproves the view of some physiologists that the heart acts in obedience to a stimulus derived from the sympathetic ganglia located in its substance.

Mechanical Causes which Arrest the Heart's Action.—In asphyxia, and in some organic diseases of the heart, we have arrest of the action of

[1] I have not made a sufficient number of experiments to be able to state this positively, as a general law; but it is certain that the general muscular irritability continues long after the heart has ceased to beat: and the question arises, when we notice the heart of a cold-blooded animal in a quiescent state, but contracting upon irritation, whether it does not contract spontaneously, but at remote intervals, as in Expt. X. This question could only be answered by more extended observations.

this organ. When this is caused by mechanical obstruction, as in disease of the aortic orifice, we attribute death to over-distension of the heart; but in asphyxia it becomes a question whether death be due to this cause, or to the circulation of black blood in its substance, as was supposed by Bichat. In experiments upon the lower animals, when we expose the heart and keep up artificial respiration, we can easily see the immediate effects of arrest of respiration upon its action. It becomes distended, changes from a red to a blue colour, showing that venous blood is circulating in its substance, and gradually its movements cease. But if respiration be recommenced before its action has been entirely arrested, it immediately becomes florid, its distension is gradually relieved, and soon its normal action is re-established. In order to determine the cause of stoppage of the heart in asphyxia, I have made the following experiments.

Expt. XV. Feb. 1, 1861.—A dog was etherized at 2.15 P. M.; the chest was opened in the usual way. At 2.25, I stopped respiration. In fifty seconds the heart became black and much distended. Respiration was recommenced, which had the effect of soon restoring normal action. The pulmonary artery and aorta were then tied suddenly with a strong cord; the heart became much distended, was of a red colour, laboured more than when respiration had been stopped, and in forty seconds it became necessary to remove the ligature for fear of permanently arresting its action. After removing the ligature, the heart gradually returned to its normal condition, *but more slowly than when respiration had been arrested for fifty seconds.*

At 2.40 a grain of woorara was injected into the areolar tissue. At 4.10 the aorta was compressed; the heart laboured, and in twenty-five seconds the compression was removed, and it gradually resumed its normal action. Respiration was then suspended with the same effect on the heart. In one minute respiration was recommenced, and the heart resumed its natural action.

In this experiment, compression of the aorta and pulmonary artery produced more trouble in the heart's action, the trouble came on more rapidly, and it was longer before its action became natural, than when respiration was stopped; though when the vessels were compressed the heart was florid, showing red blood circulating in its substance, and when respiration was arrested it became dark.

Expt. XVI. Feb. 8, 1861.—A large dog was poisoned with woorara, and the chest opened in the usual way. Respiration was stopped for two *and a half minutes.* The heart became very dark, much distended, and laboured; but when respiration was recommenced it became gradually relieved, and in a few minutes regained its normal action. The aorta was then tied for *two minutes.* The heart remained red, became more distended, and laboured more than in the previous instance, but gradually resumed its action after the ligature was removed. During the time that the aorta was compressed, here, as in Expt. XV., respiration was continued.

In this experiment I wished to ascertain how long the heart could be kept distended by asphyxia or compression of the great vessels, and yet resume its functions when the cause of the distension was removed.

Conclusions.—Great distension of the heart will produce paralysis of its muscular fibres, and this is the cause of the arrest of its action in asphyxia,

and in many cases of sudden death; not the circulation of venous blood in its substance, as was supposed by Bichat. The experiments which I have detailed demonstrate this fact with regard to asphyxia; for here it is shown that the greater the amount of distension, the sooner does the heart cease its contractions. The heart is arrested sooner by ligature of the great vessels, when red blood circulates in its substance, than by arrest of respiration, when it is supplied with black blood, because in the first instance the amount of distension is greater.

The mechanism of this muscular paralysis is the same as that of the paralysis of any striped muscle by straining. If a muscle be violently extended, as in a dislocation, we have loss of function for a period proportionate to the severity of the strain. The same is true with regard to the heart; but the constant action of the heart is necessary to existence; and when this muscle is paralyzed by straining of its fibres by distension, the animal dies before it has time to recover its functions. In case of asphyxia, then, so long as the heart continues to act, though feebly and at long intervals, artificial respiration will probably restore life; but after its action has been suspended, there can be little or no hope of restoring it by this or any other measures.

Cases of sudden death from organic disease of the heart, contrary to the popular impression, are not common; and the only form of this affection in which sudden death is likely to occur is disease of the aortic orifice. In this form of the affection, the heart is liable to over-distension from any cause which increases the force and rapidity of the circulation; and death results from stoppage of the heart, in the same manner as when we ligate the aorta, as was done in the experiments before detailed.

In death from injury to the head, as from apoplexy, respiration is interfered with, and distension of the heart occurs in precisely the same way as when we interrupt the action of the bellows in our experiments upon the lower animals. This is farther illustrated by the experiments of those observers who stun the animals upon which they operate in order to observe the action of the heart; if artificial respiration be not immediately established, the heart ceases to act from distension, and the animal dies.

In death from poisoning by opium, the respiratory muscles are paralyzed by the poison, and the heart ceases to act in the same manner in which it does in other instances of asphyxia. It would then follow that if artificial respiration be kept up until the power of the poison be exhausted, and natural respiration be gradually restored, the life of the patient would be preserved; and the well-known experiments of Sir Benjamin Brodie with opium and woorara have proved that this is the fact.

In some cases of convulsions, where death occurs, respiration is interfered with, and the heart is arrested by over-distension. This is true of all nervous diseases which, from their action upon the general system, or upon the respiratory apparatus, produce death.

In death from introduction of air into the veins, the air going to the right side of the heart is divided into minute bubbles which cannot pass through the lungs. The heart becomes distended from this obstruction, and ceases to contract from over-distension.

We thus find that distension of the heart is able, by its mechanical action on the muscular fibres, to cause stoppage of this important organ, and death; that sudden death may generally be attributed to this cause; that the cause of this distension may usually be referred to the respiratory function; and that the indications are, therefore, to re-establish this function, by artificial respiration or otherwise, when it is arrested, or to prevent the diseases under which the patient may labour from interfering with it.

Influence of the Pneumogastric Nerve on the Action of the Heart.—The heart, like other of the striped muscles, is provided with nerves derived from the cerebro-spinal system; but the action of the nerves which go to the heart differs from the nervous influence exerted upon any other muscle. If we divide a nerve which is distributed to a voluntary muscle, that muscle is paralyzed. But if we divide the pneumogastric nerve, which is distributed in part to the heart, this organ, far from being paralyzed, is accelerated in its action. Bernard has found that division of the pneumogastrics in the neck increases the number of cardiac pulsations, sometimes even doubling them; but that the force of these contractions is diminished. When we galvanize the peripheral end of a divided nerve going to a muscle, the muscle is thrown into violent contractions. But when we galvanize the peripheral ends of both pneumogastrics, the action of the heart is arrested. This experiment was made in 1845 by Weber, and has been repeatedly verified by physiologists since that time; but the cause of this peculiarity of action has never been satisfactorily explained.

In the first place, it is important to determine whether the galvanic stimulus be conveyed to the heart directly through the motor filaments of the pneumogastrics, or through the sensory filaments to the nervous centres, and, by reflex action, operates through other nerves on the heart. This is easily ascertained by dividing both pneumogastrics and galvanizing alternately the central and peripheral extremities; when it is found that the galvanic current applied to the peripheral extremities will arrest the action of the heart, while the same stimulus applied to the central ends produces no such effect. By means of woorara, the motor nerves and the motor filaments of the mixed nerves are paralyzed, and the two systems are dissected out, as it were, by this curious poison; and we find that when the pneumogastric nerves are galvanized in an animal poisoned by this agent, it is impossible to arrest the action of the heart. This fact was pointed out by Bernard,[1] and has been repeatedly verified by myself.

If both pneumogastric nerves of a dog be isolated in the middle of the

[1] Bernard, "Substances Toxiques et Medicamenteuses," p. 348.

neck, and subjected to a slight galvanic current, the first effect which we notice upon the movements of the heart, when this organ is exposed to our view, is a diminution in the frequency of its pulsations. If the current be then gradually increased in intensity, the heart becomes arrested; it remains dilated instead of contracted; and it ceases to act so long as the current is continued. When the galvanization is stopped, the heart soon commences to beat, and in a few minutes has resumed its normal movements. This effect is produced in most of the inferior animals, and can readily be shown in the frog, turtle, alligator, and other cold-blooded animals, which are well adapted to experiments on the heart and on the nerves; but in birds, Bernard has not been able to demonstrate it,[1] for what reason, he does not state. When we apply our galvanic stimulus directly to the heart, as I have done in some instances, after the organ has been removed from the chest, we put a stop, if the current be sufficiently powerful, to all regular pulsations, and have nothing but the irregular vermicular action which is observed when the irritability of this organ has become nearly exhausted. This fact I have observed in the heart of the dog.

Endeavouring to throw some light upon the cause of arrest of the heart's action by galvanization of the pneumogastrics, I instituted the following experiments upon the dog, turtle, and alligator:—

Expt. XVII.—The heart of a large dog was exposed in the usual way while the animal was under the influence of ether. After the chest had been opened, and while artificial respiration was being kept up, the pneumogastric nerves were isolated in the neck, and a feeble current was passed through them with the magneto-electric machine used in former experiments.

The heart was arrested by quite a feeble current, in the manner above described. This was repeated several times. The action of the heart commenced again when the current was arrested.

Expt. XVIII.—The heart of a turtle was exposed and found contracting regularly. The pneumogastric nerves were then isolated in the neck, and a feeble galvanic current passed through them. (This was effected by bending the ends of the conducting wires in the form of hooks and catching up each nerve.) The heart was immediately arrested. It recommenced when the current was interrupted, and stopped when it was resumed.

Expt. XIX. Mar. 13, 1861.—In a medium-sized dog, under the influence of ether, the carotids and pneumogastric nerves were exposed. The cardiometer (see fig. 3) was applied to the right carotid, and the following observations were made:—

Arterial pressure (constant) . .	(Minimum) 125	millimetres.
At each action of heart . . .	(Maximum) 130	"
Pulsations	5	"

The pneumogastrics were then divided. The movements of the heart became more rapid, and the instrument marked—

Arterial pressure (very variable) . . .	100 to 150	millimetres.
Oscillations with heart's action	2½	"

The peripheral extremities were feebly galvanized. The action of the heart became slower, and the instrument marked—

Minimum	40	millimetres.
Maximum	65	"
Pulsations	25	"

[1] Bernard, "Physiologie et Pathologie du Système Nerveux," tome ii. p. 394.

The galvanization was then arrested and the instrument marked—

Minimum	147½ millimetres.
Maximum	150 "
Pulsations	2½ "

Expt. XX. Mar. 11, 1861.—The pneumogastrics and carotids were exposed in a large dog, in which the chest had been previously opened and the heart exposed while the animal was under the influence of ether. The cardiometer was applied to the right carotid and marked—

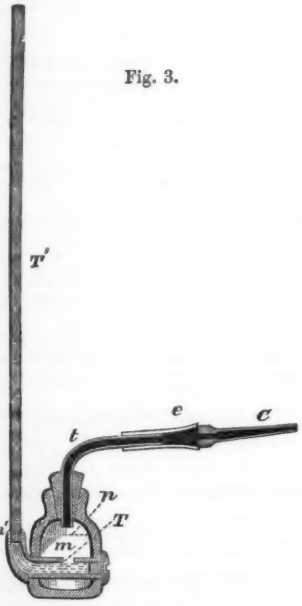
Minimum	40 to 45 millimetres.[1]
Maximum	45 to 50 "
Pulsations	5 "

The pneumogastrics were then feebly galvanized. The pulsations of the heart were diminished in frequency, and the instrument marked—

Pulsations	20 to 30 millimetres.

Experiments XVII. and XVIII. merely demonstrate the arrest of the heart's action by galvanization of the pneumogastrics in the dog and turtle. This fact is now perfectly established, and the two experiments here recorded are merely introduced to confirm previous observations.

Fig. 3.



Explanation of Fig. 3.

The cardiometer is composed of a thick and solid glass bottle, pierced by an iron tube securely soldered, and having an opening (*T*) by which the mercury which fills the bottle enters. One end of the iron tube is closed, the other projects from the bottle and bends upwards in such a way as to receive at (*n'*) a glass tube (*T'*) which is graduated, and which is at most from 1-12 to 1-8 of an inch in its interior diameter.

At the upper part, the bottle is hermetically sealed by a cork pierced by a tube (*t*) of glass or iron, at the end of which is adjusted a metallic tube (*C*) designed to enter the vessel in which it is desired to measure the pressure. The tube (*C*) is joined to tube (*t*) by a tube of vulcanized India-rubber, which must be very short.

When the instrument is in operation, all of the upper part of the apparatus (*C e t*) is filled with carbonate of soda in order to prevent coagulation of the blood. The level of the mercury is (*n*) in the bottle, and (*n'*) in the small tube. This level corresponds to zero, and when the blood presses on the surface of the mercury, the pressure is communicated by the opening (*T*) of the iron tube, and the mercury ascends in the graduated glass tube. The length of the tube (*T'*) should be as great as 250 millimetres for powerful pressures. (Bernard, *Propriétés et Altérations des Liquides de l'Organisme*, tome i. page 167.)

[1] When the chest is opened, the pulsations become more frequent and the pressure of blood is much diminished; as is seen by comparing these tables with those in the preceding experiment.

Experiments XIX. and XX. show that when the pneumogastrics are divided, and the action of the heart is accelerated, its force is diminished as measured by the cardiometer; and that, when the action of the heart is retarded by a feeble current of electricity, its force is correspondingly increased. Both the arterial pressure and the pulsations are diminished in Expt. XIX. by the administration of ether, and very much diminished in Expt. XX. by the operation of opening the chest; but my object was merely to obtain the relative pressure and pulsations, and the effects of the ether and opening the chest did not interfere with these observations, in which I wished to show that when the pulsations of the heart were increased in number, their force became diminished, and *vice versâ*.

The following experiments were made to determine the influence of woorara upon this peculiar action of galvanism applied to the pneumogastrics on the heart:—

Expt. XXI.—A large dog was poisoned with woorara, and the heart exposed in the usual way. The pneumogastric nerves were then isolated, and a current of electricity passed through them. The apparatus was the one used in the other experiments, and with the most powerful current that could be produced it was impossible to affect the action of the heart. The animal had come completely under the influence of the woorara.

Expt. XXII.—A large turtle was poisoned with woorara at 1 P. M., and at 4 P. M. was quite dead. The heart was then exposed and found contracting regularly. The pneumogastric's nerves were then isolated in the neck, and a powerful current applied with the machine before described. It was impossible to arrest, by this means, the action of the heart.

(These experiments, like Nos. XVII. and XVIII., represent numerous others of a precisely similar character.)

The experiments here detailed confirm the facts that galvanization of both pneumogastrics will arrest the movements of the heart, and that woorara so acts upon these nerves as to abolish this property. This action of woorara upon the motor filaments is in common with its action upon the other motor nerves. Having an opportunity of operating upon the alligator, and wishing to repeat these experiments, and to observe, at the same time, the action of woorara upon this animal, I did so with the following interesting results.

Expt. XXIII. Nov. 14, 1860.—An alligator, upwards of six feet long, was poisoned by the injection of about three grains of woorara under the skin of the hind leg. (This woorara was of an inferior quality, and the dose was equal to about a grain of that made use of in former experiments.) In thirty minutes he came sufficiently under its influence to be easily handled. The chest was then opened, and the heart, which was pulsating regularly, exposed. The animal was quite dead by the time that the dissection was finished. The pneumogastric nerves were then exposed in the neck and galvanized. *The movements of the heart were arrested as long as the current was continued, and recommenced when it was interrupted.* Artificial respiration was kept up for some time, but this had no effect upon the action of the heart, and was only done to exhibit the play of the lungs. The animal was kept under observation three or four hours, and the foregoing singular fact repeatedly verified.

Expt. XXIV. Jan. 28, 1861.—The above experiment was repeated upon another alligator of about the same size as the first, but poisoned with the first

quality of woorara. In making the dissection for exposing the heart, a small nervous filament going to the sterno-mastoid muscle was exposed, irritation of which with the scalpel induced contraction of the muscle, though the animal was quite dead. The pneumogastric nerves were exposed, and the heart's action arrested by a moderate galvanic current. The animal was kept under observation upwards of three hours, and this point repeatedly verified.

Twenty-four hours after, the heart was still beating vigorously, and could be arrested by galvanization of the pneumogastric nerves as before. The nervous filament going to the sterno-mastoid muscle was galvanized, which produced slight contractions of the muscle. Muscular irritability was very marked.

It is only in the class of birds that experimenters have met with any peculiarity with regard to the influence upon the heart of the galvanization of both pneumogastric nerves; and in this class, in proportion to their elevation in the animal scale, has it been difficult, and in most cases impossible, to arrest the action of the heart by the means which will invariably produce this effect in mammals, and more easily, even, in the cold-blooded animals. Nor have any animals been found able to resist the influence of the woorara upon the motor nerves, with the exception of the alligator. These observations should be extended to alligators of small size; but, as yet, I have not been able to procure them.

When I first noticed the phenomenon which I have described, I was at a loss to account for it; for I saw that the alligator was perfectly motionless and insensible, and that the woorara had been tolerably prompt in its action, the animal coming under its influence in from thirty minutes to three-quarters of an hour, or an hour; which, in a large cold-blooded animal, where the processes of life are languid, is as soon as we would expect. In carefully reviewing, however, my observations, I found that, in the alligator, the nervous system was much less affected by this remarkable agent than in other animals; and that while in dogs the motor nerves become entirely paralyzed, and death actually takes place by arrest of the muscles of respiration, in the alligator, when voluntary movement and the cerebral functions are abolished, so that the animal can be operated upon without the slightest difficulty, the motor nerves are still somewhat irritable. The reader will see, by reference to Experiment XXIV., that a nervous filament was exposed during the operation, and still retained its irritability as manifested by muscular contractions when it was irritated by the point of the scalpel. This persisted and was marked, though in a less degree than before, twenty-four hours after death. The properties of the pneumogastrics remained with no sensible diminution.

It is evident that the motor functions of the pneumogastric nerves, especially those connected with the heart, are more important to life than those of the ordinary motor filaments which are distributed to the general muscular system; and we can appreciate the wisdom of a provision that this nerve should be protected from disturbing influences, like the action of poison, to a greater degree than others. We see evidence of this in the numerous sources from which the pneumogastric derives its motor filaments;

anastomosing, as it does near its origin, with the spinal accessory, the facial, the sublingual, and the first and second cervicals. But we must not be satisfied with this purely anatomical explanation, if it be possible to determine its powers of resistance to poisonous agents experimentally. To do this we must employ some means of retarding the absorption of woorara, observe its effects upon the nerves of the animal from the first, and determine whether its action upon the general motor system precede that upon the pneumogastric nerve. For this purpose I instituted the following experiments, the observation on the alligator not being in itself satisfactory.

Expt. XXV. Feb. 1, 1861.—A medium-sized dog was etherized, and the chest opened in the usual way. The operation was performed at 2.15 P. M., and at 2.40 a grain of woorara, dissolved in water, was injected under the skin of the thigh. The sciatic nerve and both pneumogastrics were then isolated and galvanized. By this means, convulsive movements were produced in the leg, and the heart was promptly arrested by a feeble current.

At 3.10 the sciatic and pneumogastrics were galvanized, producing convulsions in the leg, not so marked as before, and arresting the action of the heart.

At 3.40, one hour after the injection of the woorara, the sciatic was found insensible to galvanism, but the heart could be arrested by galvanization of the pneumogastrics, though it required a powerful current. A weaker current diminished the frequency, and increased the force, of its pulsations.

At 4.10 the sciatic was still unexcitable, but a powerful current applied to the pneumogastrics arrested the action of the heart.

Expt. XXVI. March 11, 1861.—One grain of woorara of an inferior quality dissolved in water was injected under the skin of a medium-sized dog. Twenty-five minutes after, no effect being produced by the first injection, a second grain was introduced. In ten minutes signs of poisoning were manifested, the posterior extremities became partially paralyzed, and the animal was placed upon the operating table. The chest was then opened and the heart exposed, the animal manifesting no signs of pain. The pneumogastric nerves were then isolated and galvanized, promptly arresting the action of the heart. After the observation had been continued for about thirty minutes, the animal partially recovered from the influence of the woorara, and made some voluntary movements.

Aided by these experiments, we are able to understand the reason why, in the alligator, galvanization of the pneumogastrics continued to arrest the action of the heart when the animal had been poisoned with woorara. The alligator, when undisturbed, breathes very slowly; and, like other cold-blooded animals, pulmonary respiration is not necessary to the movements of the heart. Woorara seems, then, first to act upon the brain, abolishing sensation and voluntary motion, before the motor nerves are paralyzed. This was demonstrated in Expt. XXV. on the dog, for the effects of the ether which was administered in making the dissection were allowed to pass off, and the woorara which was subsequently administered abolished sensation and voluntary motion long before the motor nerves were much affected, as demonstrated by galvanization of the exposed sciatic. The general motor nerves then slowly came under its influence, and last, the pneumogastrics, though their function was not entirely abolished. By the means employed in this operation, injecting the woorara after the vital powers had been enfeebled by opening the chest, and exposing the thoracic

organs to the cold air, the dog was approximated to the condition of the alligator.

In Expt. XXVI., an inferior quality of woorara was employed, which abolished the functions of sensation and volition, but had not sufficient power to entirely paralyze the general motor nerves, and had little or no effect upon the pneumogastrics. The specimen used had the well-known properties of ordinary woorara, but was deficient in power.

From these observations we can make the following deductions with regard to the action of woorara upon the nervous system. It affects volition and sensation, in whatever part of the central nervous system these functions reside, and the motor system of nerves. With regard to the order in which various parts are affected, we have, first, sensation and voluntary motion coming under its influence; then the general motor system of nerves; and last, as the preceding observations have demonstrated, the motor elements of the pneumogastric nerve, especially those which affect the heart. By an ingenious series of experiments with this substance upon frogs, Bernard has demonstrated the fact that woorara affects the motor nerves exclusively, leaving the sensory filaments intact, as well as the muscular system. He has also shown that the sympathetic system is paralyzed, for the abnormal heat and congestion which are developed in the ear of the rabbit, for example, when the sympathetic is divided, and which is reduced to the normal standard when the cut extremity is galvanized, is abolished when the animal is put under the influence of this agent.[1]

In the beginning of the section devoted to the influence of the pneumogastrics upon the heart, I mentioned that, though the phenomena which follow galvanization of this nerve are very well established, no explanation of them has yet been given which is generally received. It is plain that we must first thoroughly understand the cause of the action of the heart before we can explain a modification of this process, and this has been the great obstacle in the way of such an explanation. It now seems to me pretty well established that we cannot go farther, in explaining the cause of the heart's action, than to attribute it to an inherent and peculiar irritability of its muscular fibres, and not to the influence of the little ganglia which are found in its substance. Not only is it impossible to prove this latter supposition, but it seems to me that it can be disproved. Then we cannot take the ground which is assumed by some, that the heart is under the influence of two sets of nerves; one from the sympathetic system, which accelerates its action, and the other from the cerebro-spinal system (the pneumogastric), which retards it. Many other explanations have been offered by physiologists, but it is not my object to discuss them here. I

[1] For a full exposition of these facts, the reader is referred to Bernard's "Leçons sur des Substances Toxiques et Medicamenteuses," and the "Physiologie et Pathologie du Système Nerveux."

will simply endeavour to give the actual state of our information on this subject, as deduced from the experiments detailed in this essay, and others which are generally received and acknowledged.

1. The heart possesses in its own fibres the property of intermittent contraction, and the stimulus of the blood passing continually through its cavities regulates to a certain extent its movements. This is shown by experiments in the section on the "Cause of the Rhythmical Contraction of the Heart." Numerous observations on the heart, after section of both pneumogastrics in the neck, show that these nerves further regulate the heart's action; for when their influence is cut off, the pulsations of the organ become rapid and feeble. This is shown in Expt. XIX., where the pneumogastrics were divided in the neck, increasing the rapidity of the heart's action, but diminishing the force of its contractions, as indicated by the cardiometer. Many instances of palpitation of the heart may undoubtedly be referred to a deficiency in proper innervation transmitted to it through the pneumogastrics; for most of them may be referred to derangements of the nervous system, and frequently these derangements have their origin either in the lungs, from the individual being put "out of breath" by exercise, or, in the stomach, from indigestion; both, organs which are abundantly supplied with filaments from the pneumogastric nerves. The phenomena which accompany palpitation of the heart are precisely those which are produced by section of those nerves.

2. When we galvanize the pneumogastrics in the neck, the galvanic current is not conducted by the nerves to the heart, but we imitate, by means of galvanism, the ordinary "nervous force," an irritation from which is conveyed to the muscular fibres of the heart. It is difficult, when the irritability of the pneumogastrics is unaffected, to regulate the stimulus so as to observe the effects of slight action of these nerves upon the heart, its muscular fibres being extremely sensitive to irritation of any kind; but by the action of woorara, when it only partially paralyzes the nerves, as in the alligator, or in the dogs used in Expts. XXV. and XXVI., we can determine that a slight stimulus diminishes the frequency but increases the power of the contractions of the heart; but a powerful stimulus paralyzes the fibres so long as it be kept up. Expts. XIX. and XX. show that when the number of the pulsations of the heart is diminished, their force is increased. If we apply the galvanism directly to the heart, we can equally paralyze its fibres; and in this instance, if the current be sufficiently powerful, this is permanent, and we have no more regular contractions of the organ. The heart, like other organs, is subject to various changes in its nutrition, and if it were not under the control of, and regulated by, the pneumogastric nerves, would be subject to variations in its action which would seriously affect the general system. A certain amount of nervous force, like the "muscular sense" which produces tonicity of the muscular system, is continually supplied to it by the cerebro-spinal system, which regulates and moderates its action; it can be

closely imitated by electricity or galvanism; a slight current merely moderates the action of the heart, but a powerful current, which represents the force in an intensely exaggerated form, arrests its action completely. We cannot be surprised that an organ which possesses such peculiarity in the properties of its muscular structure, and the proper action of which is so important to the well-being and the life of the animal, should be thus under the dominion of, and guarded by, the nervous system. There are instances on record of immediate death by stoppage of the heart from fright, anger, grief, and other severe mental emotions which operate powerfully on the nervous system. Syncope from these causes is by no means uncommon. In the latter instance, where the heart resumes its functions, the nervous shock carried along the pneumogastrics has been sufficient only to temporarily arrest the action of the heart; in the former, when death is the result, the shock has been so great that the heart is unable to recover from its effects. Thus we explain no phenomenon in physiology, however impractical it seem at first, without finding a useful application in pathology.

Mechanism of the Closure of the Valves of the Heart.—The four ventricular orifices of the heart are furnished with valves which permit the blood to flow in only one direction. In some of the inferior animals the auricular orifices, by which the blood passes from the veins into the heart, are provided with a valvular apparatus. This is the case in fishes; but in animals which have a double heart, and in man, the openings of the great veins into the right auricle, and of the pulmonary vein into the left auricle, are provided with no valvular apparatus. These orifices are narrowed, however, by the contraction of the fibres during the auricular systole, moderating, though not entirely preventing, regurgitation; while the play of the auriculo-ventricular valves permits the blood to flow freely in and fill the ventricles. The ventricles then contract powerfully, close the auriculo-ventricular valves, force open the semilunar valves, and project the blood, on the one side, into the pulmonary artery, and on the other into the aorta; from whence it is immediately prevented from regurgitating by the closing of the aortic and pulmonary valves. Thus the blood, forced into the right auricle from the veins of the system, moves but in one direction, towards the aorta, and is prevented from taking a backward course by the valves which protect the orifices of both ventricles. It is correctly stated by physiological writers that the tricuspid valves, unlike the mitral, do not entirely close the right auriculo-ventricular orifice. This may be observed by a very simple experiment. If we take the fresh heart of any animal, the bullock, for example, cut away the left auricle, and force water into the ventricle with a syringe introduced into the aorta, the aortic valves being previously destroyed, it will be seen that the mitral valve effectually prevents the egress of the water through the auriculo-ventricular opening; and that these valves, the action of which may thus be beautifully exhibited, are closely

and effectually coadapted by the pressure exerted against them. But if an analogous experiment be performed upon the right side of the heart, cutting away the right auricle so as to expose the tricuspid valves, and injecting fluid against them through the pulmonary artery, it will be seen that a certain amount of regurgitation takes place, and that these valves do not effectually close the auriculo-ventricular orifices. Mr. T. King, in an essay published in *Guy's Hospital Reports* for 1837, pointed out the peculiarity of action of the tricuspid valves, and called it the "safety-valve function of the right ventricle." He stated that it was a provision to prevent congestion of the lungs when anything occurred to obstruct the pulmonary circulation; and it is evident that by this means we have the delicate tissue of the lungs, in which congestion cannot be relieved by anastomoses, as in the general circulation, protected from injurious accumulation or pressure of blood. The difference between the action of these valves on the two sides of the heart I have repeatedly verified, and it can be easily demonstrated in the manner just described.

But the next question which presents itself is: By what means are the valves of the heart made to close? This question may easily be answered with regard to the semilunar valves. The blood circulates in the arterial system under a pressure which will support a column of about six feet of water, or six inches of mercury. During the flow of blood from the ventricle into the aorta, the power of the heart overcomes this pressure, and opens the valves; but when the force of the heart is taken off, the valves are closed, effectually preventing regurgitation. We would naturally suppose that the auriculo-ventricular valves were closed in the same way, by backward pressure; and this indeed is the general opinion among physiologists; but, in 1843, Dr. Baumgarten, a German physiologist, endeavoured to prove by experiment that these valves were closed by a current in another direction, attributing it to a contraction of the auricles, and not the action of the ventricles. I do not know that this view has met with much favour among physiologists, but some have confirmed his experiments, and the explanation has been adopted by Milne-Edwards[1] and a few others. The experiments upon which this view is based are briefly these:—

The heart of a large warm-blooded animal is prepared by completely removing the auricles, so as to expose the auriculo-ventricular valves. It is then held in a vertical position, the valves lying in the cavity of the ventricles, leaving the orifice patent. If water be poured slowly into one of the ventricles through this opening, the valves will gradually float out and their edges approximate; then, when the ventricle is nearly filled, if the stream be suddenly increased in power, the valves completely close.

The facts here stated are entirely correct, and I have repeatedly verified

[1] Milne Edwards, "Leçons sur la Physiologie et l'Anatomie comparée de l'Homme et des Animaux," tome iv. page 30.

them; but if, as before stated, a stream of water be forced *against* the valves, the orifice is closed. We cannot, therefore, reason from the experiments of Baumgarten that this is the natural mechanism of the closure of the valves, but must examine closely into the conditions as they exist in the normal condition of the organ. For that purpose, and with the object of settling this question, if possible, I carefully repeated the experiments of Baumgarten, and carried his observations a little further.

Expt. XXVII. Jan. 30, 1861.—In this experiment I found that the mitral valves were closed when the current of water poured into the ventricle flowed in a small stream; and in this case it is evident that they were closed by backward pressure, for the current of water, flowing thus, on a bullock's heart prepared in the manner above described, did not exert pressure upon the whole of the auricular face of the valves, but merely made a small opening for itself between them. I then employed a larger stream, and in this instance the valves were overpowered, and the water flowed in a full stream from the aorta. The aortic opening was then closed, and regurgitation took place freely at the auriculo-ventricular orifice. In this modification of the experiment, however, the force of the water was considerably greater than that of the natural current of blood; it was impossible, indeed, to graduate this, and to pour the fluid into the ventricle in a stream which would impinge upon the entire surface of the valves which did not flow with a great deal of force. These facts were repeatedly verified in this, and confirmatory experiments, which it is unnecessary to detail here.

Let us now examine the pressure of the blood in the cavities of the heart during circulation; for it is evident that when the auricle is entirely removed, and water is poured from a height into the ventricle, we are far from fulfilling the natural conditions, which it is so necessary to observe in all physiological observations. During the circulation, the veins, heart, and arteries are completely filled with blood; no air or gas can exist, except in solution, in the circulatory system; and especially in the heart does the presence of any gaseous fluid disturb the circulatory function. The blood also circulates under a certain pressure; and a certain force is exerted by the heart at every pulsation. In the arterial system the pressure of the blood is represented by a column of six inches of mercury. This pressure is constant in the arteries, but intermittent in the heart. In the heart the pressure is null during the diastole, as has been shown by actual experiment,[1] but nearly one-third greater than the arterial pressure during its systole. In the venous system the pressure is much less constant than in the arterial; it is always less, and subject to numerous variations. Bernard found the arterial pressure in the carotid of the horse, measured by his cardiometer, to be 110 millimetres; at each cardiac pulsation it was increased to 175.[2] In another horse he found the pressure in the jugular to vary between 105, 100, 95, and 90 millimetres.[3] The pressure exerted by the contraction of the auricles has never been ascertained. Though it adds some-

[1] Bernard, "Leçons sur les Propriétés Physiologiques et les Alterations Pathologiques des Liquides de l'Organisme," tome i. page 173.

[2] Bernard *o . cit.*, page 172. [3] Bernard, *op. cit.*, page 203.

thing to the venous pressure, it cannot be very considerable. Under these circumstances, during the diastole of the ventricles, the venous pressure operates upon the auricular face of the auriculo-ventricular valves, it has no cardiac pressure to oppose it, and the orifice is kept patent. The same is true during the contraction of the auricles, the pressure is thereby increased, it is exerted by a column of blood which impinges upon the entire surface of the valves, and the ventricles are thus completely filled. During this time the blood is prevented from regurgitating from the aorta and pulmonary artery by the semilunar valves which are closed by the arterial pressure. But when the ventricles act, they exert a force sufficient to overcome the arterial pressure, which keeps the semilunar valves closed; and they close at the same time the auriculo-ventricular valves, producing one element of the first sound of the heart. The systole of the ventricles ceases; its pressure is taken off; the arterial pressure closes the semilunar valves, producing the second sound; the venous pressure opens the auriculo-ventricular valves, and keeps the orifice patent, until the succeeding contraction of the ventricles. Thus it is that the cardiac pressure, intermittent and operating during the systole of that organ, being superior to the arterial and venous pressure, at the same time opens the semilunar, and closes the auriculo-ventricular valves.

Inasmuch as Baumgarten's experiments showed that the auriculo-ventricular valves, which are closed by means of a backward pressure during the systole of the ventricles, can be closed by pouring a stream of water into the ventricles from the auricles, it occurred to me to extend these observations to the aortic semilunar valves, and for this purpose I made the following experiment:—

Expt. XXVIII. Jan. 30, 1861.—A bullock's heart was prepared so as to exhibit the action of the aortic valves; which was done by cutting away a portion of the left ventricle so as to expose them to view, securing the nozzle of a large syringe in the aorta, and forcing water towards the ventricular cavity. The semilunar valves were thus closed, effectually preventing the passage of the fluid. While the nozzle was yet in the aorta, diminishing, but not preventing, the flow of fluid, water was poured from a considerable height into the vessel. The valves were at first floated out, and then closed in the same manner as in Experiment XXVII., on the mitral valves. This observation was repeatedly verified.

This last experiment would go as much to prove that the semilunar valves are closed by a current from the ventricle into the aorta, as the preceding one does that the current from the auricle to the ventricle closes the mitral valves; yet, I will venture to assert, no one could entertain, for a moment, the view that the force which overcomes the resistance of the aortic valves, by operating from the ventricles, closes them by the same current. The experiment, of course, proves nothing with regard to the action of the valves at the aortic orifice, but it shows the falsity of conclusions, drawn from experiments, in which natural conditions are so utterly disregarded, as in those which favour the idea that valves arranged for the

purpose of permitting the flow of blood in one direction, and preventing reflux, are closed by the opposite current.[1]

Seat of the Sensation of the "Besoin de Respirer," which gives rise to the Movements of Respiration.—The circulation of the blood is intimately connected with the function of respiration. In health the number of pulsations of the heart bears a definite relation to the number of the respiratory movements; when the pulse is increased in frequency, we breathe more rapidly; and when the heart labors, as in cases of advanced disease of this organ, the patient experiences a sense of suffocation which is not dependent upon the condition of the respiratory organs. What gives rise to this sense of suffocation? Why is it, when the lungs are unaffected, when a large supply of pure air is taken in at every respiratory act, that a sense of suffocation attends imperfect action of the heart? These are questions which are of vital interest to the pathologist; but they cannot be answered unless we ascertain the seat of sensation of want of air, which ordinarily is not perceived by the brain, but insensibly induces respiratory movements; but when circulation or respiration is much disturbed, gives rise to the distressing sensation of suffocation. To resolve these questions, which are as yet imperfectly and incorrectly answered by physiologists, is the object of this division of the present paper.

Respiration is considered to be a reflex function, in which a certain impression is conveyed to the respiratory centre, and is followed by the action of the respiratory muscles. This is partly under control of the will; but under ordinary circumstances is involuntary. It is unnecessary to enter into any discussion with regard to the location of the respiratory centre. Physiologists now agree that it is situated in the medulla oblongata, at about the origin of the pneumogastric nerves. Almost the same unanimity exists with regard to the localization of the impression which gives rise to the respiratory act; attributing it to the impression made upon filaments of the pneumogastric nerves by the carbonic acid in the smallest lung cells. This is the view which was advanced by Marshall Hall, and which is now generally adopted. But there are difficulties in the way of explaining all the phenomena which we observe in health and disease by this supposition. In diseases of the heart, we may have dyspnœa, and yet the air be rapidly and effectually changed in the lungs. The evolution of a large amount of carbonic acid by the lungs, if it be promptly exhaled, does not produce dyspnœa. And in experiments upon the lower animals, which are to be mentioned hereafter, phenomena are developed, for which such an explanation will not

[1] Since this paper has been written, the author has seen a short article on the method and time of closure of the auriculo-ventricular valves, by Dr. Halford, but the experiments seem to prove nothing beyond those here mentioned, being, indeed, little more than repetitions of them.

suffice. There are some physiologists, indeed, who do not accept this explanation of Marshall Hall; among them are Bérard, who locates the sense of want of air in the heart;[1] John Reid, who thought that the respiratory movements were due to the action of the black blood upon the medulla oblongata; Volkmann and Vierordt, who thought that these movements were reflex, and due to the excitation of the general sensory system of nerves by venous blood. But this is a question which may be settled by direct experiment, and that is the mode in which it is to be considered here.

If a dog be rendered insensible by ether, and the chest be opened, artificial respiration being kept up by a pair of bellows, he will make no respiratory movements so long as respiration is carried on artificially; but soon after we stop this, the diaphragm, intercostals, and other respiratory muscles, which are actually denuded and exposed to our view, will be seen to contract violently, this contraction, or effort at respiration, ceasing so soon as the artificial respiration be resumed.

An experiment analogous to this was performed in 1664, by Robert Hook, in which he demonstrated that respiratory efforts ceased in an animal so long as the requisite quantity of air was supplied to the lungs. In this experiment, he made an opening into the pleural cavity and the lungs of a living dog, and forced a current of air through the trachea and out at the artificial opening. So long as the current was continued, the animal remained quiet; but when it was interrupted, he made efforts at respiration. This experiment is made use of by Marshall Hall, to support his doctrine of the reflex character of respiration, the excitation coming from the lungs; for he says that so long as fresh air was supplied to the lungs, there was no stimulus for respiration, and therefore no efforts were made; but when this current ceased, or when carbonic acid was substituted for atmospheric air, the contact of the carbonic acid, which, in the one instance, was exhaled by the venous blood, and in the other was introduced into the lungs, produced the excitation which was necessary to the respiratory act. This, however, is but a superficial view of these phenomena. We must examine into the condition of the heart, and rest of the circulatory system. We know how the heart's action is dependent upon respiration, and how an arrest of the interchange of gases in the lungs is immediately felt by the circulation. It was for the purpose of observing these conditions that the following experiments were instituted.

Expt. XXIX. Feb. 16, 1861.—A medium-sized dog was etherized, and the heart and lungs exposed in the usual way. A pair of bellows was introduced into the trachea and artificial respiration kept up. As long as this was performed, the animal made no efforts at respiration, even after he had almost recovered from the effects of the anæsthetic; but when the artificial respiration was stopped, he soon began to make efforts to breathe, as demonstrated by contractions of the diaphragm and intercostals, which were exposed to view. The femoral artery was then isolated and divided, a ligature applied to the distal

[1] Bérard, Cours de Physiologie, tome iii. p. 523.

end, and the cardiac compressed with the fingers so that the blood could be permitted to flow at will. A small stream was then allowed to escape, which was of the brilliant red colour of arterial blood; the animal remaining quiet and respiration being kept up actively. *Respiration was now stopped, and the animal remained quiet until the blood became dark in the exposed artery. He then, and not until then, began to make efforts at respiration. Respiration was now resumed and the blood gradually became red. The animal continued to make efforts at respiration, till the blood became red in the artery.* This observation was frequently repeated, and the above phenomena were invariable. Since that time, also, I have repeated the experiment upon other animals, always with the same result.

Expt. XXX. Feb. 19, 1861.—A good sized dog was poisoned with woorara, and the chest opened in the usual way. When the animal came fully under the influence of the poison, he ceased all respiratory movements. Artificial respiration, however, was kept up for three hours; and in about two and a half hours, he had so far recovered from the effects of the poison as to make efforts at breathing when artificial respiration was interrupted. The femoral artery was then opened and divided as in the former experiment. *When respiration was arrested the animal made efforts to breathe, but only when the blood became dark in the artery, and ceased these efforts when it became red again on resuming respiration.* This observation was made repeatedly.[1]

Expt. XXXI. Feb. 15, 1861.—In a large dog, under the influence of ether, in which the heart was beating regularly, the organ was suddenly cut from the chest. The animal afterwards made several respiratory movements. In this instance the "*besoin de respirer*" could not be derived from the heart, as it had been removed from all connection with the system.

Expt. XXXII. Mar. 11, 1861.—A good sized dog was etherized and the heart exposed in the usual way. Artificial respiration was actively kept up, and while the heart was pulsating regularly and vigorously, it was cut from the chest by a single sweep of the knife. The lungs were still regularly inflated, *but in thirty seconds the animal commenced to make efforts at respiration, which were continued for two and a quarter minutes.* These efforts were powerful and convulsive.

It would seem settled by these experiments, that the "*besoin de respirer*" which is conducted to the respiratory centre, and excites the movements of respiration, is not located in the lungs, nor in the heart, but in the general system; and the sense of suffocation is due to the presence of black or venous blood in tissues which should be supplied with arterial blood. We would therefore expect that this peculiar sensation would, if it resided in the general system, be conveyed to the nervous centres by the ordinary sensory nerves, and not by the pneumogastric, as was supposed by Marshall Hall. This, indeed, is the fact, and it can easily be proved by the following experiment:—

Expt. XXXIII. Feb. 15, 1861.—A medium-sized dog was etherized, and the heart and lungs exposed in the usual way. The occurrence of respiratory efforts when the blood became black in the arteries, and their cessation as soon as it

[1] This experiment has additional interest as confirming the well known experiments of Brodie, in which he demonstrated, that in poisoning by certain substances, their effects will pass off, if artificial respiration be continued for a certain length of time. Among these poisons are woorara and opium. Of course, in this instance, it would have been impossible to preserve the life of the animal, after the chest had been opened and the thoracic organs exposed, but he evidently recovered considerably from the effects of the poison.

regained its red colour, was noted. The pneumogastric nerves were then isolated in the neck and divided, producing the usual effect upon the movements of the heart. The experiments of arresting artificial respiration and exciting respiratory efforts on the part of the animal, were then repeated with precisely the same effect as before division of the pneumogastrics, and as observed in other experiments.

Though we have located the sensation which induces respiration in the tissues, and shown that it does not reside in the organs of respiration themselves, we have not arrived at the cause of this sensation. The venous blood either irritates the system from the presence of elements which are not contained in the same proportion in arterial blood; or the tissues feel the want of some principle which the venous blood does not contain in sufficient quantity. The great difference between venous and arterial blood is in the quantity of oxygen which they contain. According to the latest experiments of Bernard with regard to the comparative quantity of oxygen in arterial and venous blood, it is shown that the arterial blood of a healthy dog contained 18.28 parts of oxygen for every hundred parts of blood; while venous blood contained only 8.42 parts of oxygen per hundred.[1] In these experiments, Bernard found that when the gas was estimated by displacement with hydrogen or nitrogen, it became diminished if it were allowed to stand a few hours, and that part of the oxygen became united with carbon to form carbonic acid. He employed the oxide of carbon as a displacing agent, which prevented this change, and hence the large proportion of oxygen which he found in both varieties of blood. The usual estimates are based upon the experiments of Magnus, and indicate in the arterial blood of five animals (three horses and two calves) separately examined, a mean of 2.44 per cent. of oxygen for arterial, and 1.15 per cent. for venous blood; an estimate very far short of the truth. The venous blood is supposed by Brown-Séquard and others to be an active stimulant to the tissues on account of its irritating properties; but when we examine the arterial blood and find that it contains such a large proportion of oxygen, which we know is indispensable to the system, and is contained in small quantity in the non-arterialized blood, our immediate inquiry is, as to whether the excitation in question be due to the stimulating properties of the venous blood, or the want of oxygen in the tissues, which can be supplied only by arterial blood. This question I conceive can be settled by experiment. An animal does not feel the "*besoin de respirer*" while artificial respiration is kept up actively; but he does soon after this process is interrupted. In this case, partially oxygenated blood circulates in the arteries and is supplied to the systemic capillaries. If it be that the tissues simply need oxygen, any cause which would prevent oxygen from coming in contact with them would give rise to respiratory movements, though there were no black blood in the arteries, and though an abundant supply

[1] Bernard, "Leçons sur les Propriétés Physiologiques, et les Altérations Pathologiques des Liquides de l'Organisme," tome i. p. 367.

of fresh air were kept up to the lungs. The following experiment bears upon this point.

Expt. XXXIV. Feb. 19, 1861.—A good sized dog was etherized and the chest opened in the usual way. Artificial respiration was established and Expt. XXIX. verified. The blood was then allowed to flow freely from the femoral artery, while artificial respiration was actively continued. While the blood continued to flow, the respiratory muscles were carefully observed. During the first part of the bleeding no respiratory efforts took place; *but when the blood had flowed for a considerable time, and the system was becoming drained, respiratory efforts commenced, feeble at first, but as the bleeding continued, becoming more violent until the whole muscular system was affected by convulsive movements.*

This experiment is one of exceeding interest and importance. By the withdrawal of blood while respiration was active, the tissues were deprived of oxygen from a diminution in the quantity of blood, and relieved from the stimulation of the black blood, if it have any stimulating properties, for all the blood which goes to the capillaries is of a pure arterial character. No stimulation, then, is applied to the tissues; they simply are deprived of their normal supply of oxygen by a diminution of the oxygen-carrying fluid. This gives rise to the "*besoin de respirer*" first to a slight extent, but as the hemorrhage continues, increasing in intensity till the whole muscular system is convulsed from the overpowering sense of suffocation; a sense which we imagine we feel in the lungs, but really resides in the general system.

These experiments give us a new view of the "*besoin de respirer*" which gives rise to the respiratory movements, and of the sense of suffocation which is incident to the interruption of these movements. More and more as we advance in our knowledge of the functions of the body, do we take away certain sensations which we have been accustomed to perceive in special organs, and locate them in the general system.

In treating of the sensation now under consideration we are led to compare it with various others with which we are familiar. The system needs periodical rest; it is undergoing an incessant waste which must be supplied by food—a continual loss of fluid which must be supplied by water; and needs a constant supply of oxygen which is furnished by respiration. These are wants of the general system; but we refer their indications to particular parts. Drowsiness is indicated by drooping of the eyelids; hunger by uneasiness in the stomach; thirst by dryness of the mouth and fauces; and the "*besoin de respirer*," and sense of suffocation, when respiration is interfered with, is referred to the lungs. But the sensation of hunger does not reside in the stomach, though it may be momentarily arrested by the introduction of substances, even of an indigestible character, into its cavity. When we observe a patient suffering from any disease which is characterized by deficient digestion and assimilation, while the system is capable of feeling the want of nourishment, an abnormal appetite is a characteristic symptom; and the hunger is not appeased for any length of

time by the introduction of food into the stomach. This, as is well known to practical physicians, is a frequent symptom of diabetes and chronic diarrhœa. Direct experiments have been made upon the sensation of thirst. Magendie and Bernard have kept an animal (a horse) without water for twenty-four or forty-eight hours, divided the œsophagus so as to divert food and water from the stomach, and then allowed him to drink. As fast as the water was swallowed it flowed out at the wound; and though the mouth and fauces were moistened, the thirst was not satisfied and the animal continued to drink. Bernard has made analogous experiments with dogs in which he had established gastric fistulæ. These experiments I have frequently repeated, and as they are very striking and easy of execution, I report an example:—

Expt. XXXV. Nov. 17, 1860.—A dog which had been operated upon for the establishment of a gastric fistula two days before, was kept without water for twenty-four hours. At the time of the experiment he was quite lively, having suffered little from the operation. The cork was then removed from the tube in the stomach, and the animal was allowed to drink. He drank until he desisted from actual fatigue, and after resting for a moment drank again in the same way, the fluid all this time flowing freely from the fistula. This was repeated several times until the animal gave up the effort in despair. The cork was then replaced in the tube, and when the animal drank, his thirst was soon satisfied.

These experiments, which are well known to physiologists, show that thirst is a sensation felt in the tissues, but referred to the mouth and fauces; and that though these, and the walls of the stomach, be continually moistened, the thirst is not appeased; nor can it be until the fluid has been taken into the bloodvessels and circulates in the system. This want of fluid is always manifested by animals after the withdrawal of blood. I have repeatedly observed animals from which I had removed blood by the jugulars go to the water and drink copiously as soon as they were set at liberty.

Conclusions.—Respiration is a reflex phenomenon, under ordinary circumstances, and movements connected with it are due to an impression conveyed from the general system to the medulla oblongata, whence a stimulus is sent out which animates the respiratory muscles. While respiration is carried on effectually, without exertion to an animal, as in artificial respiration, evidently no impression is made upon the respiratory centre, for no movements take place.

The impression which excites respiratory action is received from the tissues and not from the lungs; for it is only when dark blood, instead of red, is supplied to the tissues, that we have the impression conveyed to the respiratory centre, producing efforts at respiration.

This impression is not transmitted through the pneumogastric nerves, but through the general sensory nerves; for there is no difference in the manifestation of respiratory movements when the supply of air to the lungs is cut off, if both pneumogastrics be divided.

This impression is due to the want of oxygen in the tissues, and not to stimulating properties of the venous blood; for when the supply of oxygen

is cut off by abstracting blood from the system, the phenomena which we noticed as occurring during interruption of respiration are marked, though air be supplied in abundance to the lungs.

This impression is not due to distension of the cavities of the heart, as suggested by Bérard; because the heart may suddenly be removed from the body of a living animal, and the respiratory efforts will occur as in the case of abstraction of blood.

This impression (the sense of want of air, "*besoin de respirer*") when exaggerated constitutes the sense of suffocation; and it, like the sense of fatigue, of hunger, and of thirst, has its usual source in the general system, though it manifests itself to us in the lungs in the same way that we have fatigue affecting the eyelids, hunger the stomach, and thirst the mouth and fauces. They are all indications of wants of the system, and cannot be effectually relieved by the local effects of anything upon the organs to which they are referred by our sensations.

The necessity for respiration, or for oxygen, then, exists in the tissues; and asphyxia cannot be solely applied to arrest of the function of the lungs, but to anything which interferes with the consumption of oxygen by the system. Anything which operates in this way gives rise to a sense of suffocation, and consequently to general convulsions, if it be carried sufficiently far. We can thus explain various pathological phenomena which would otherwise be obscure. The operation of simple asphyxia by tying the trachea, or preventing air from gaining entrance into the lungs, induces the sense of suffocation which first gives rise to respiratory efforts more violent than ordinary, and subsequently to general convulsions. We are all familiar with these phenomena, however they may be explained.

In the poisoning by oxide of carbon we have general convulsions which arise from the sense of suffocation; for this agent so operates upon the blood-globules, that though they continue red, they are rendered incapable of performing their function of supplying oxygen to the system.

In poisoning by hydrocyanic acid, when the system is not immediately overpowered by this agent, and the muscular irritability destroyed, the blood becomes incapable of supplying oxygen to the system, and convulsions ensue as the result of the sense of suffocation.

In death by hemorrhage, convulsions, occurring just before death, are invariable. This is also the result of deficient supply of oxygen to the tissues, and the sense of suffocation is the starting point. This was demonstrated in Expt. XXXIV., in which the animal was bled to death.[1]

Finally, in all cases where the supply of oxygen is cut off, not from the lungs, but from the tissues, a sense of suffocation is the result, and convulsions ensue following upon violent efforts at respiration.

[1] These convulsions have been explained in various ways by physiologists, but never satisfactorily, though they have long been noticed.

Résumé.—In the foregoing essay, I conceive that I have established the following facts, which are either not generally admitted or not understood by physiologists:—

First. That the heart elongates during the systole of its ventricles.

Second. That the cause of the rhythmical contraction of the muscular fibres of the heart is resident in the fibres themselves, is one of their inherent properties, and remains so long as they retain their "irritability." That it is derived neither from the cerebro-spinal nor sympathetic system of nerves.

Third. That the natural stimulus which excites the regular and effectual movements of the heart, is the blood, and that this cannot be replaced by a fluid of less density.

Fourth. That though the flow of blood in the cavities of the heart is sufficient to induce, under ordinary circumstances, regular contractions of the organ, still it is necessary that these movements be further regulated and controlled; and that this is effected through the agency of the pneumogastric nerves.

Fifth. That the action of the heart may be arrested, through the motor filaments of the pneumogastric nerves, by means of galvanism; that this does not take place in animals poisoned by woorara, on account of the paralysis of the motor nerves. That the motor filaments of the pneumogastrics are the last which are affected by this agent, and that in the alligator they are left almost intact. That the cause of the arrest of the heart by galvanization of the pneumogastrics is the exaggeration of the force which regulates the action of the heart, rendering it slower and more powerful.

Sixth. That in asphyxia, the cause of the arrest of the action of the heart is over-distension of its cavities; and that anything which brings about a sufficient amount of distension will equally arrest the action of this organ.

Seventh. That the auriculo-ventricular valves are closed by a backward pressure operating during the contraction of the ventricles, and not by the current of blood from the auricles to the ventricles.

Eighth. That the excitation which gives rise to the reflex phenomena of respiration is received from the general system, and not from the lungs or heart. That this excitation is due to the want of oxygen in the tissues, and not to stimulating properties in the venous blood. That the exaggeration of this excitation constitutes the sense of suffocation, and gives rise, if excessive, to general convulsions.

APPENDIX. *Some Points in the Anatomy of the Circulatory System of the Crocodilus Mississippiensis, or Alligator.*—The anatomy of the alligator is imperfectly, and, in many respects, incorrectly described in most works upon Natural History. The description of the circulatory apparatus, however, given by Milne Edwards,[1] in his great work on *Physiology*

[1] For a full description of the circulatory system of the alligator, see Milne Edwards, "Leçons sur la Physiologie," etc., tome iii. p. 424 et seq.

and the Comparative Anatomy of Man and the Inferior Animals, now in course of publication, is very accurate; and as the arrangement of the heart and larger vessels is very peculiar in this animal, differing, indeed, from any other of which we have any description, I have thought that a sketch of these peculiarities might not be uninteresting.

The heart is quite small in proportion to the size of the animal; and, like the organ in reptiles generally, the ventricular portion is small in proportion to the size of the auricles. The position, shape, etc., of the auricles do not differ from those of other reptiles; except that the right auricle is much larger than the left; but when we examine the ventricular portion, we find that it is divided by a complete septum into two chambers, a right and a left ventricle, like the heart of a warm-blooded animal. From the ventricles arise the aortæ; one from the left, and one from the right side; we also have a pulmonary artery going to the lungs from the right ventricle. The right aorta passes immediately over to the left side, and, as it carries venous blood, may be called the venous aorta; while the left, or arterial aorta passes directly over to the right side. There is no communication between either auricles or ventricles upon the two sides; but at the commencement of the two aortæ is an opening which permits an admixture of the venous and arterial blood to a limited extent. This is called the foramen of Pinazza, because its discovery was erroneously supposed to belong to him. It was really described by Hentz, an American anatomist, in 1824, in a paper published in the *Transactions of the American Philosophical Society;* while Pinazza described it in 1833. Following out now the distribution, etc., of these two aortæ, we have the arterial aorta first giving off a large branch, the brachio-cephalic, which almost immediately gives off the left subclavian, going to the left superior extremity. The brachio-cephalic divides into the left subclavian, already mentioned, and a single carotid artery which goes in the median line to the base of the skull, there divides into two vessels, which soon bifurcate and form the external and internal carotids of the two sides; the internal going to the encephalon, and the external to the muscles, etc., about the head. Next is given off the right subclavian artery distributed to the right superior extremity. Each subclavian artery, a short distance from its origin, gives off a small cervical artery which goes to the head, and is accompanied by the jugular vein and the pneumogastric nerve. It is by being found with these vessels that we recognize this nerve. These are the principal branches which are given off by the arterial aorta alone. The venous aorta gives off no branches in the neck, but passes back to the vertebral column, anastomoses by a branch of considerable size with the arterial aorta, and sends a branch, larger even than the anastomosing branch, to some of the abdominal viscera, which are thus supplied with venous blood. The dorsal aorta is formed by the union of the arterial aorta, and the anastomosing branch of the venous aorta, and thus carries mixed blood; it passes down the back, gives off in its course the intercostals, the anterior mesenteric, the renal arteries, the vessels of the posterior limbs, the posterior mesenteric, and finally is distributed to the tail.

The distribution of the blood in this animal is very singular. The fore-legs, the head, and face, are supplied with almost pure arterial blood, as the communication by the foramen of Pinazza is very imperfect. There is but a single carotid artery in the neck, and the pneumogastric nerves are found accompanying the cervical arteries which are given off by the subclavians. Part of the abdominal viscera, the stomach, liver, spleen, etc.,

are supplied with venous blood. The kidneys, intestines, hind legs, and tail are supplied with a mixture of arterial and venous blood.[1]

The length of time for which the nervous and muscular irritability of these animals is retained after death, renders them very valuable in many physiological experiments. I have found that, when poisoned with woorara, this irritability persisted for days. For a considerable time, four or five days after death, even when the weather was quite warm, no decomposition took place. I do not know that this apparently antiseptic property of woorara has ever been remarked, but it certainly seemed to retard decomposition in the alligators I have experimented upon.

ART. IV.—*On the Formation of Knots on the Umbilical Cord.* Read before the Obstetrical Society of Boston, May 4, 1861. By WILLIAM READ, M. D., formerly Physician to the Boston Lying-in Hospital. (With eight wood-cuts.)

OBSTETRIC writers, with very few exceptions, have attributed the formation of the knots which are occasionally found on the umbilical cord, at birth, to a change in position of the fœtus while in the uterus. This may take place either from its own movements, or from some movement communicated to it by the mother. In this way, passing through a loop of the cord, it forms the knot, which may be tightened by the further gyrations of the fœtus, or remain loose till delivery. Practically the subject is of no importance. The very nature of the complication effectually prevents any measures to protect the fœtus from the dangers which may arise if the knot should tighten. But while granting all this, and admitting that nothing we can do will prevent their occurrence, it may be well to examine into the evidence upon which the general belief, as to their formation, is based; to determine whether it rests upon good foundation, or is one of those fallacies, so many of which, like the rubbish which disfigures the proportions of many a stately fabric, are to be found in the writings of obstetric authors, and which have been handed down from one generation to another, and passed current because they have never been questioned.

Smellie[2] states the fact of their occurrence, without attempting any explanation of their formation. Dr. R. Lee[3] says: "True knots are sometimes made by the fœtus upon its own umbilical cord, and it thereby unconsciously,

[1] The description of the heart and arteries, which is here given, is nearly, if not precisely, according to the views entertained by Dr. Bennett Dowler, of New Orleans, who has made such extensive researches into the anatomy and physiology of the alligator. These views, however, have never been fully published by him, but were verbally communicated to the writer.

[2] Coll. 19, No. 2, case 3. [3] Mid., p. 124.

if I may so express it, commits suicide by compressing the vessels. I have seen two instances of this, and in both the knots must have been tied before the labours commenced." Rigby[1] states that "the manner in which these knots are formed may be easily imagined, when by chance the cord lies in the form of a ring, and the fœtus happens to float through it, a noose is made, which, when drawn tight, by accident, forms a knot." Churchill[2] says of the funis: "Occasionally, owing to the movements of the child at an early period, it may be coiled round its neck, tied in knots, or escape below the head, so as to prolapse during labour." Montgomery[3] entertains the general belief of their formation while the fœtus is yet in utero. Prof. Miller[4] remarks that the coiling of the funis about the neck "affords the key to an explanation of these knots, at least when they are single. They are tied by the fœtus slipping through the circle about its neck." Levret[5] merely mentions the fact of their occurrence, and states that, when they do occur, "the infant ordinarily perishes before birth, or is born very much emaciated." Baudelocque[6] says: "They sometimes form themselves during pregnancy, and at an early date, but there are also cases where the cord does not knot itself, until the instant when the child issues from the womb of the mother." Mauriceau[7] mentions having delivered eight children with knots upon the funis of each, which he says were formed during the intra-uterine life of the child. Velpeau[8] remarks: "On some occasions they are real knots, either simple or complex; more frequently, however, they are doublings, vascular nooses, whether of arteries or of the veins; the former are met with particularly where the cord is very long, are owing to the movements of the fœtus, are effected in the same way as the twisting of the cord about the neck, limbs, or other parts of the child, frequently met with in parturition, and, it may be said, are but the definite result of this last mentioned disposition." Blundell[9] says: "To Dr. Hunter I may refer you for a very plausible explanation of the formation of these knots when single, for he has suggested that the umbilical cord, at birth, may, perhaps, form a coil round the margin of the os uteri within, and that the fœtus, in passing the orifice of the uterus, may at the same time pass through the loop, carrying the umbilical extremity of the cord along with it, so as to form a knot at the very moment when the body passes into the world. And this explanation enables us to understand well enough how a single knot may be formed; but then how is it that two or three knots are produced? how that a knot may be found on the cord in the earlier months, though the fœtus has never left the cavity of the uterus?" Prof. Meigs[10] explains

[1] Mid., Chap. III.
[2] Mid., Am. ed., p. 181.
[3] Signs and Symptoms of Pregnancy, Am. ed., p. 541 et seq.
[4] Obstet., p. 151.
[5] Art. des Accouch., p. 305.
[6] Art. des Accouch., p. 518.
[7] Obs., p. 133.
[8] Transl., by Meigs, 4th Am. ed., p. 202.
[9] Ed. by Castle, p. 108.
[10] Obstet., 2d ed., p. 218.

their formation, by supposing that "if in its gyrations within the womb, the child should enter a coil of the navel-string, and, passing through it, should thus make a knot on the cord—that knot happening to be strongly drawn, might cause its death by hindering the complete return of the blood of the placenta." Cazeaux[1] says: "When the cord is very long, it very often shows one or more knots. Some of these are formed during pregnancy, and often at a very early date; but others are never formed except at the moment of delivery. They never can be drawn tight enough (during pregnancy) to compromise the life of the child, to whose movements they are certainly owing. But we can understand how the cord, shortened by its circumvolutions about the trunk or the neck, may find itself stretched with great force. The knots will then be drawn tight enough to interrupt the circulation entirely. If the labour is prolonged, the death of the child will be the result." Ramsbotham[2] says: "Sometimes the funis itself is found in labour to be twisted into a loose knot; but this appears to me to be produced rather by the movements of the fœtus in utero, than to exist as an original conformation." Davis[3] says: "We occasionally find a knot on the string, which in all probability, and in the greater number of cases, is found at the time of labour. It may, however, happen at any other time, in cases where there might be an excessive quantity of liquor amnii, as, for example, we find mentioned in Smellie's cases. Most frequently, however, this knotting takes place during labour; and it is very easy to conceive how this may happen. Let the cord be supposed to be lying in a coil round the orifice or neck of the womb; the head of the child having to pass in that direction, will find its way through it, and must necessarily draw it into a knot." Hunter[4] says: "If I have not been deceived, I have seen it twice." Rokitansky[5] says: "The true knots of the umbilical cord, that occur in rare cases, are of importance, as they may occasion obstacles to, or a cessation of, the circulation in consequence of the traction exerted upon them during parturition." The inference from this is, that he believed them to be formed during the intra-uterine life of the fœtus, or *before* the time of parturition.

They may be single, or double, or more than one on the same cord.

Usually when they occur, the cord is found free from any convolutions about the body of the child, but, in rare cases, where the cord is extraordinarily long, both these conditions have been noticed. Baudelocque[6] mentions a case where he found a triple knot on the cord, which was thirty-six or thirty-seven inches long, and moreover twice round the neck of the child. The position of the knot he estimated at about a foot from the umbilicus.

[1] Accouch., 5th ed., Paris, 1856, p. 205. [2] Process of Part., Am. ed., p. 82.
[3] Prin. and Pract. of Obstet. Med., p. 833.
[4] Anat. Descript. of the Gravid Uterus.
[5] Vol. ii. p. 350, Pub. of Sydenham Soc. [6] Loc. sup. cit., § 521.

He also states that it was drawn as tight as any other knot could have been in like circumstances. The child was, however, born alive. Smellie (*loc. cit.*) mentions a case where the funis was "nine hand-breadths long, had a loose knot on it, and was twisted round the neck of the child, which was dead." Dr. Lee[1] instances a case where the knot was "ten inches from the umbilicus, and the cord also surrounded the neck of the child once." In this case the cord was much compressed where the knot was tied. Another is quoted by the same author, in which the cord was "atrophied between the knot and the umbilicus." "I have no doubt," says Dr. Lee, "these were both formed early in pregnancy." In both these cases, the context warrants the opinion that the children were dead. Dr. Coale[2] reports a case where there were two knots on the cord, about five inches apart, one of which was double, and the other single—the latter being nearest the placenta. Prof. D. H. Storer[3] mentions a case, quoted from a foreign journal, in which the "patient, seven or eight months advanced in pregnancy, being under a bed for some purpose, was suddenly surprised in that position, and hastily backing out, immediately had the symptoms of a dead child, of which she was delivered ten days after. A knot was found in the cord, so tightly drawn as to fully account for the death of the child by impeding the circulation." Dr. Zollickoffer[4] reports two cases, in both of which there was a knot on the funis, which was coiled about the neck and chest.

Knots may occur when the cord is comparatively short.

In a case of my own—Mrs. S., third pregnancy, April 24, 1860—I found a single knot on the cord, about midway its length. The child, a full-sized boy, was perfectly healthy and vigorous. The cord was twisted in both directions very strongly. To the right—the exceptional way—from the umbilicus to the knot; and to the left—the ordinary and most frequent direction—from that point to the placenta. The entire length of the cord was a little short of twenty-eight inches; exceeding the child's length by about six inches only. In another case, three weeks later than the preceding (May 13), the cord was thirty-three inches long, and the knot, which was first noticed lying loosely encircling the left ankle, tied back towards the umbilicus, so that when it was drawn tight by the tension brought on it in withdrawing the child from the mother, it was situated so near the umbilicus as to make it impossible to apply the ligature without untying it. This was also a simple knot. In another case still (Oct. 5, same year), the cord was fifty-four inches long, very firmly twisted, and as large as the forefinger, with a single knot, about the middle. The child was vigorous, weighed thirteen pounds, and its condition forbade the supposition of any diminution of nourishment by constriction of the cord.

By some writers much importance has been given to the complicity of

[1] Mid., p. 124.
[2] Records Bost. Soc. Med. Imp., vol. iii. p. 190.
[3] Ibid.
[4] Am. Journ. Med. Sci., July, 1841, p. 109.

the knots described. Montgomery,[1] in alluding to this point, says: "The formation of the single knot, as it is generally called, is not difficult to conceive; but there is one of a very curious kind, of which I have seen but two instances, and it is so complicated, and requires for its formation so much dexterity and management to imitate it, that I feel at a loss to understand how it could have been formed in utero." The knot referred to, is what is usually called the figure of 8 knot, and is the same as that to be seen in the specimen presented by Dr. Coale to the museum of the Medical College in this city. The knot figured by Baudelocque is still more complicated; a knot in this case having been tied on a knot. Dr. Lee, in commenting on this knot (*loc. cit.*), remarks: "Baudelocque gives a figure of a double knot on the cord. These appear to consist of two knots formed at different times. The fœtus must have passed twice through a large loop of the cord." By this opinion, adding his testimony to the impossibility of such a knot being tied at any other time, or in any other way.

Fig. 1.

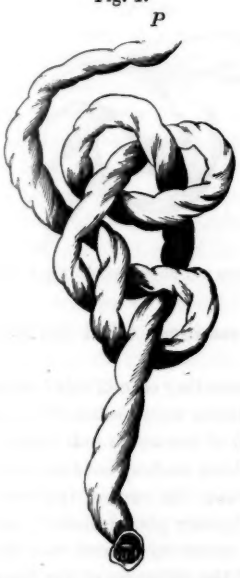


Fig. 2.

BAUDELOCQUE'S KNOT.—Fig. 1 represents the knot in its first stage of formation; *P*, placental end. Fig. 2, the knot as found at birth; *P*, placental end.

Equal in complexity with this knot figured by Baudelocque, is a knot noticed by Dr. George H. Lyman, of this city, a figure of which is here

[1] Signs and Symptoms of Preg., loc. cit.

given. In this case the cord was thirty inches long, and was round the neck and one of the arms of the child. The knot it will be seen is double; a knot being tied upon a knot, as one would tie a string twice in the same place.

Fig. 3.

Fig. 4.

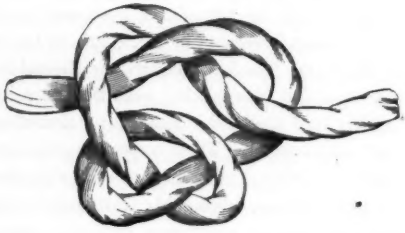
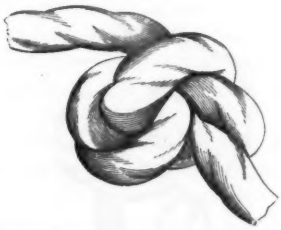


Fig. 5.



DR. LYMAN'S KNOT.—Fig. 3. Knot in the stage of formation; *P*, placental end. Fig. 4. Intermediate form. Fig. 5. Knot as it was found tied at birth.

The funis of one child, in twin cases, is sometimes found tied in a knot round that of the other.

Mr. William Newman reports a very interesting case of this kind in the *Edinburgh Monthly.*[1] In this case the children were contained in one bag of membranes, and were born at an interval of two and a half hours. The first was born alive, the second dead. "About midway between the umbilicus and the placental attachment of the funis, the cord of the first child, which was readily distinguishable by the ligature placed upon it, was tied in a single knot, and passing through the noose so formed, was the cord of the second child, which, on account of the tightness of the knot, was completely strangulated. With the exception of some flattening at this point, the cords presented a perfectly healthy appearance. The children were equally mature and well nourished, so that the circulation through the cords could not have been obstructed for any length of time before death. It has already been stated that the nurse, before my arrival, had

[1] July, 1858, p. 8.

exercised some force upon the cord of the first child, by drawing it downwards, from which I conclude that the constriction probably took place at that period." The length of these cords was about twenty-four inches each.

The appearance of this knot on the cord is represented in Fig. 6, *A* being the cord of the first child, *B* of the second.

Fig. 6.

The knot figured by Mr. Newman who offers two solutions of its mechanism, makes no exception to the rule of formation proposed in this paper. "By the first, it may be supposed that at an early period of pregnancy the cord *A* must have been twisted into a single loop; through this loop the fœtus of cord *A* must have passed, and in place of going through it in part only (as when the cord encircles the neck in ordinary cases), it must have passed completely through; the noose still remaining so far loose as to permit the second fœtus with its cord *B* to drop through it.

"The second view may be that cord *A* may have made a single twist around cord *B*, the subsequent noosing of cord *A* being due to the formation of a knot upon it after it had already encircled cord *B*."[1]

The latter of these two explanations is incontestably the true one. The rotation of one child round the other, in a single sac of the membranes, without the necessity of any extraordinary movements, will bring its funis in such a relation to the other that all the conditions requisite for the formation of the knot will be then present, needing only the passage of the child through the loop thus formed, to complete and tie the knot at delivery. Complicated as the fact of the knot being tied round the funis of the other child, *apparently*, makes the case, in reality it is no more complex or incomprehensible, nor does it require any other conditions for its formation than are necessitated to tie a single knot alone.

As to the knot reported by Stein, which is quoted by Mr. Newman[2] and called a "real knot," no figure is given. The impossibility of imitating, on a cord, such a knot, without so much manipulation as seems to be forbidden by the circumscribed capacity of the uterus in such a case, induces

[1] Loc. cit.

[2] Mr. Newman, in addition to his own case, quotes a case from Siebold's *Lucina*, 1806, vol. iii. part 1, No. 144, p. 313, also of twins, "which had only reached the seventh month, and were putrid: they were inclosed in a common bag of membranes and had one placenta. Close to the placenta the two cords were tied together in a real knot, and the one was afterwards twisted round the other several times."

a doubt whether the knot was so "real" as is reported. A half knot can be tied with the utmost ease—more easily even than when there is but a single cord—but a complete knot, in which both parts are perfect, as may easily be proved by experiment, is a difficult operation to succeed in.

The condition of the cord at the place of the knot, has been brought forward by some writers to prove the date of their formation, whether at delivery or earlier in the pregnancy.

Dr. Priestly[1] says that, "early in pregnancy, when the fœtus is small, and the liquor amnii is proportionately large, knots may be formed on the cord. Later in pregnancy also, or at delivery, they may also be formed, if the cord is very long. The difference between those formed early, and at birth, is, that the former cannot be untied and straightened out, while the latter can." (See also Dr. Lee's cases, quoted *ante*.)

The data thus presented are sufficient to show that it is the common belief of the profession that knots on the cord may be formed during the intra-uterine life of the child, as well as at the time of delivery. The evidence upon which this opinion is based may be divided into two kinds: 1st, that derived from the state or condition of the fœtus, and the cord at the place of the knot: 2d, the complexity of the knot itself, and the supposed difficulty of its being tied at the time of delivery, when the child passes through the organs of the mother.

And 1st: As to the evidence derived from the condition of the fœtus at birth:—

In regard to this part of the evidence, many writers, without questioning its validity, have assumed that in all those cases where the child has been stillborn, with a knot on the cord, its death was the result of such obstruction.

But, in reality, this is no proof at all, for the same condition of the fœtus—*i. e.*, stillborn—is observed very much more frequently when there are no knots on the cord. The final decision, therefore, whether the death was a coincidence only, or the actual result of the knot, must be determined on other grounds.

For still stronger reasons, if the condition of the fœtus at birth, whether alive or dead, when a knot is found on the cord, cannot be brought in evidence that the death resulted from the constriction of the cord by the latter, it cannot be offered in proof as to the *time when the knot was tied;* for in the first place, children are born alive with knots tightly drawn on the cord, and in the second place, children are born putrid, with the skin peeling off, and in all respects like what Smellie (see *ante*) reports, in many instances where there is no obstruction to the circulation of the blood through the cord, in consequence of a knot having formed on it. Now, when similar results occur under different conditions, or different results under similar conditions, it is a fair inference that they cannot stand to each other in the

[1] Med. Times and Gaz., Feb. 26, 1859; also, Lect. on the Gravid Uterus, p. 75, Lond. 1860.

relation of effect and cause. That is to say, if children are found both alive and dead, under exactly the same circumstances, so far as the knot is concerned, the question as to the effect of the previous formation of the knot in causing the death must be settled upon the probabilities of the case, upon the validity of the proof whether a knot can be thus tied in utero, and vice versâ; unless it can be reasonably inferred that the death resulted from the knot on the cord, no proof can be deduced from its occurrence, that it was tied while the child was alive.

And here, perhaps, is the turning point of the whole matter; for, if by any reasonable proof it shall be decided that knots can be tied in utero, the death, when they are found, may fairly be attributed to them with just as much reason, as in their absence to any other cause.

We come next to the evidence derived from the condition of the cord at the knot, whether compressed or otherwise, or so firmly fixed in the shape of the knot that it cannot be made to return to its former state before the knot was tied.

In regard to this point Dr. Priestly (see *ante*) asserts that "the difference between those (*i. e.*, knots) formed early and at birth is, that the former cannot be untied and straightened out, while the latter can."

If we take a fresh cord, before it becomes cold and has lost what little vitality it possessed, and tie a knot in it as tight as may be, and leave it in this condition till it becomes thoroughly dead, till it is in the same state that the body is in when it has become stiff with cadaveric rigidity, and which, in so small a body as the funis, is reached in a very short space of time, not exceeding a few minutes, we shall find when we untie the knot, that it no longer returns to its former round, pulpy, and ordinary form; it is thin, ribbon like, and condensed, and has a strong tendency to retain the curved and curled shape which it held when in the knot. In other words, it is in precisely the same state, which Dr. Priestly considers to be a proof that the knot had been tied for an indefinitely long time. The same result is obtained by tying a ligature round the cord, or subjecting it to any force which will compress it while still alive, and which is not removed till the fluids within its substance have become cold and coagulated by the loss of its vitality. The condition of the cord at the knot cannot, therefore, be quoted in evidence as to the time when the knots were tied.

And, moreover, it needs but a moment's reflection or consideration to satisfy any one that if sufficient tension should be made on the cord in utero to draw the knot tight and compress the vessels so as to produce the condition mentioned by Dr. Priestly, it would of necessity stop the fœtal circulation, and the child would die. But, as before remarked, the children are not always dead, and therefore the knots could not have been drawn tight before birth. The only exception to this tightening of the knot which could occur, would be in those cases where the cord is so long as not to be put on the strain, after the body of the child had passed through the

bight which forms the knot. But the shortness of the cord in many of the instances reported, negatives this supposition; and in those cases where the cord is so long as to be coiled round the neck once, twice, or even oftener, such as the cases reported by Dr. Zollickoffer, Dr. Lee, and Baudelocque (see *ante*), the fact that at the first turn of the child's body the knot would be drawn tight, and in consequence the child become at once asphyxiated and die, render the conclusions drawn from such premises unworthy of credence; since in those very cases the coiling of the funis went on after the first strain on the cord, to the extent of a second entire coil about the neck in the one case, and about the neck or chest in the other, and yet both children were born alive.

Dr. Lee (see *ante*) remarks that the knot in one of the cases he reports, must have been tied a long time before delivery, for the cord was atrophied between the knot and the umbilicus. But if the knot was drawn so tightly as to obstruct the circulation, the child could not have lived long enough in utero, after the knot was tied, to have given time for any such state of the cord to have become evident, or any such condition of the child to have developed itself as would have resulted from a lack of nourishment for a period of time.

Following out the division of the argument, we come next to consider the nature of the proofs derived from the complexity of the knots, as bearing upon the question of the possibility of their being tied at any other time and in any other way than by the child while in utero.

The intricate nature of some of the knots which have been recorded and figured (*e. g.*, that of Baudelocque, quoted by Montgomery and Lee (*ante*), a copy of which has been already given in this article, and the figure of 8 knots, so called, also mentioned by Montgomery (see *ante*) and reported by Dr. Wm. E. Coale of this city), seem to have inspired the greatest reverence in writers, who have held them up as being among the incomprehensible things of midwifery, the result of an inscrutable cause.

But before we proceed further in discussing this part of our subject, it will be well to consider another question as a preliminary step to its more perfect solution. That is to say, What are the probabilities, taking into consideration the condition of the fœtus in utero, that it can pass through a loop of the cord and form the knot during its intra-uterine life?

What are the facts? To tie the knot, two conditions are essential: 1st, that there be a loop; 2d, that the child should pass through it. But so long as the fœtus remains suspended by the cord, or until the time when the cord has acquired a length sufficient to allow the child to rest on the walls of the uterus, a loop can by no possibility be formed; and after this time, in order to form the knot, the fœtus must pass entirely through the bight of the loop, and clear it on the other side. Now, although the fœtus in utero may revolve on its axis, and extend itself by kicking and straightening out its limbs, is it supposable, is it within the bounds of

probability, that there is room enough in the uterus for the acrobatic display necessary to tie a knot, or form it even, on the cord?

In the case quoted by Prof. Storer, which is the only one on record in which any such connection between cause and effect, as at first glance seems to be there apparent, has been proposed, a second thought will show that the conclusion is scarcely warranted by the premises. If we take into consideration the conditions—patient seven or eight months pregnant, the child of course proportionately filling the cavity of the uterus—it seems almost beyond the bounds of possibility that a loop on the cord of sufficient size (five inches at least in diameter, fifteen inches in circumference) to allow the child to go clear through, could have been formed there; or that room enough for the fœtus to go through it, and turn and draw the knot tight, could have been afforded within the abdomen of an ordinary sized woman.

And again, if all this could take place, there are other reasons which add to its difficulty. If loops should form on the cord, they would, from the weight of the cord itself, naturally fall to the lowest part of the uterus, wherever, for the time being, that might be, and we can hardly conceive of any other arrangement. They might slip about, and slide from side to side, but the structure of the cord does not possess sufficient firmness to keep it upright in the form of an open loop, even were there room for it in the uterus.

If it be objected to this view of the question, that in the earlier periods of pregnancy, before the fœtus acquires bulk enough to largely encroach on the intra-uterine space, all the movements necessary for the formation of a knot can be easily made, it may be answered that before the tenth or twelfth week, by which time the fœtus has gained nearly one-third of its final size, and has by this very growth rendered any free movements in utero, except that of rotation, difficult, there is no twist in the funis; and until this takes place, it will not form loops from which knots can be tied, but only flexures. This view is corroborated by all the specimens of knotted cords that have been inspected, which show that the twist in every one of them is strong and decided, and has apparently thrown the cord into the kinks which form the foundation structure of the knots.

But it may be said that cases are reported on unquestionable authority, in which at the birth of the child the cord has been found tied in the groove of a limb almost separated or amputated in utero, and which must have been the effect of the stricture from the knot long before tied. Montgomery, in his essay upon "the Spontaneous Amputation of Fœtal Limbs in Utero," appended to his great work on the Signs and Symptoms of Pregnancy, discusses this subject at length, and has collected together a great number of cases bearing on this point. Though hardly firm in his convictions, he inclines to the belief that the pressure of the cord acting in this way, is one of the causes of the amputation. In Case No. 37, quoted from Siebold's Journal (Bd. xvii. st. 2, 1838), Montgomery reports that

Dr. Schwabe found "the umbilical cord wound round the right leg a little above the ankle, where it *formed a knot*, by which the development of the parts was completely prevented, and the foot nearly separated from the leg."

It is not worth while to question the fact here noted—that when the child was born the cord was found knotted in the groove of the limb—even if the authority was not unquestionable; but by what kind of proof can we connect the two together as cause and effect? Can we suppose that a degree of constriction sufficient to have caused the amputation, could have been exerted on the limb by the cord, and in the form of a knot also, with one part crossed over the other, adding compression to extension, without interfering so much with the circulation in the cord as to kill the child? As regards the ability to keep up the circulation under direct tension of the vessels, the difference between a simple coil and a knot is very great indeed, and it is much more probable that the knot was caught and retained where it was found at birth, after it had been tied by the child during its egress from the uterus passing through a loop, than that it had been previously formed on the limb in utero, and by its constantly exerted pressure caused absorption of the integuments beneath it. And in this connection it is worthy of notice, that in the great majority of the cases reported, the lesion attributed to the pressure of the cord is not the only one found. Various deformities, often uniting in the same case, show that the tendency is to abnormal growth or arrest of development, either of which would produce the results attributed to the pressure of the cord.

But if the pressure of the cord exerted on a limb produces such evident effect, why should it not always bring about the same result? Why should not the same amount of pressure, in every case, produce absorption of the integuments at the place where the pressure is made? Why is it that no instance of amputation of the head, or signs of decapitation, have ever been noticed in cases—and they are by no means rare—where the cord is once, twice, or even oftener round the neck? Or, at least, if the neck, for wise and good reasons, should escape, why does not the cord leave some depression, or mark of its contact, along the course it took across the clavicle, up one side of the neck and down the other? Certainly the integuments of the fœtus are alike throughout, and why a result will follow certain causes in the one case, and not in the other, it is impossible to understand.

It appears strange that no attempt should have been made, by actual experiment, to imitate these knots. But such, so far as any record exists, is really the fact. No one has thought that, by very simple and easy manipulations, the most complex and reduplicated knots could be imitated at pleasure, and in the very way also in which they are probably tied—by the child at delivery, and at no other time, slipping through a bight (to use a nautical phrase) of the cord lying round the os uteri.

Prof. Miller (see *ante*) very summarily explains the whole matter, by supposing that the child, after the cord has coiled round its body, slips through, and thus ties the simpler kinds—for the complex knots he has the same reverence that inspires all the rest. But this is not so. It is impossible to tie a knot, so long as the cord remains simply *coiled* about the body or neck of the child. Suppose this to be the case—the child slips through, and the cord comes straight without any complication, in every attempt. But let a loop be formed in the cord, *i. e.*, let one part of the funis be crossed on the other, and the same passage of the child through it ties the knot, invariably and at once, differing in complexity just in proportion as we increase the number of turns on itself in the loop.

To make my meaning more clear, take a string, one end of which is fixed to some object, representing the placenta, and the other free, but which shall represent the umbilical end of the cord. If now we take up a loop of this string, and turn it once on itself, so as to bring its two parts across each other, and pass the umbilical end of the string through the bight thus formed, we form and tie the single knot, as represented in Fig. 7.

If we twist the loop once more, that is, twice on itself, and proceed as before, we get the double, square, or figure-of-8 knot, so called, which we have here. (See Fig. 8.)

Fig. 7.

Fig. 8.

Fig. 7. SINGLE KNOT.—*P.* Placental end of funis. *U* Umbilical or fœtal end of funis.
Fig. 8. DOUBLE OR FIGURE-OF-8 KNOT.—*P.* Placental end of funis *U* Umbilical or fœtal end of funis.

If we take two loops, each with a single turn and as near to each other as may be, and pass the free end of the string through them, we have formed the combination which, when drawn tight, exactly imitates Dr. Lyman's knot. (See *ante*, for illustration of this.) If one of the loops has a double turn on itself and the other but one, we get the knot figured by Baudelocque, and already drawn and explained. In this way one, two, three knots, or more, provided the length of the cord suffices, may be tied upon the same cord, and at various distances from each other, if instead of tying them together in one complex knot, each one as fast as it is tied is allowed to come through the other loops and tie in reversed order, the lowest loop on the cord forming the last and uppermost knot when they are finished.

If the argument thus set forth is sound, and a case does not occur in which while yet in utero a knot is found tied upon the umbilical cord, and of which, so far as any records at hand have been consulted, no instance has been found, the opinion held by many members of the profession as to the mode in which they are produced is erroneous, and the only way they can be tied, and the time when they can be formed, must be by the passage of the child through a loop of the umbilical cord lying around the os uteri when it is ushered into the world.

ART. V.—*On Veratrum Viride as an Arterial Sedative.* A mutual paper made up of contributions from members of the Massachusetts Medical Society. By E. CUTTER, M. D., TRUMAN RICKARD, M. D., and WM. INGALLS, M. D., Committee of the Middlesex East District Medical Society.

IN the October (1858) number of the *American Journal of the Medical Sciences*, the "Veratrum Viride Committee" of the Middlesex (Mass.) East District Medical Society published a paper, entitled "*Veratrum Viride as an Arterial Sedative;* a mutual paper made up of contributions from the members of the Middlesex East District Medical Society, Massachusetts." It contained a brief history of the use of the remedy in years gone by; its botanical characters; its physical properties; its chemical composition; the time for collecting the root; the mode of preparing the tincture; its therapeutical effects; and the literature of the medicine. Following this was the general and special testimony of the members in respect to its action as an arterial sedative, the latter being presented by reports of cases.

Having presented about four hundred two-ounce phials of the tincture of veratrum viride to the Fellows of the Massachusetts Medical Society, at

the annual meeting in May, 1857, we addressed a circular to the recipients as follows:—

SIR: Please to communicate, at as early a day as possible, any facts you may have derived from the use of the veratrum viride. We hope not to seem importunate, but we beg to remind you that we are endeavouring to prepare a report upon this article which shall, in some degree at least, be commensurate with the implied hopes contained in the circular accompanying the bottle of tincture we presented to you last May. We are desirous of obtaining any general and special evidence relative to the value of this agent as an arterial sedative from reports of cases in which it was used—including the dose, the frequency of its exhibition, and the effects upon the pulse from day to day, whether favourable, unfavourable, or inappreciable; together with any collateral testimony as to its value otherwise than as an arterial sedative.

Please address either of the undersigned.

Respectfully yours,

EPHRAIM CUTTER, M. D., Woburn,
TRUMAN RICKARD, M. D., Woburn,
WILLIAM INGALLS, M. D., Winchester,
Committee of the M. E. D. M. Society.

WOBURN, April, 1859.

In response to this circular we have received thirty letters from gentlemen not members of our District Society. Some of these letters state frankly that the writers were very sceptical in respect to the ascribed virtues of the veratrum viride, and had made no fair trial of it; others show that, although they have used it, they were afraid to trust in it, and consequently so combined it with other remedies, that the particular effect of either could not be stated; others have given it a fair trial, and express themselves very highly pleased with the results. And such, we are confident, will always be the case, when an intelligent discriminating use is made of the article. We know very well that there are cases where it fails of producing the ordinary good results to be expected from it; but this militates nothing against its value. The same is true of all our most prized remedies. Why, then, should any one throw the veratrum aside as worthless, because it did not do all he anticipated in the first or second trial? The fault may have been in some idiosyncrasy of the patient, or in an inadequate dose for that particular case. Were there no exceptions, did the medicine *never* fail, it would indeed be a specific. But the experience of the whole medical world tells us that no specific has as yet been discovered, and it is foolishness for any one to expect that there ever will be. A use of the article will show that it is as certain of producing its peculiar effects, as calomel, or any other agent of the materia medica.

The statements contained in the letters we have received are mostly general. We regret that more *cases* were not reported in full, as these show clearly and definitely the effects produced by the medicine. But the fact that intelligent, observing men have been exceedingly pleased with the effects of the veratrum viride, should inspire as much confidence in the minds of the sceptical, as cases recorded in detail. If they will not believe the one, neither would they be convinced by the other.

From the letters we derive the following testimony as witnessing to the

value of the veratrum viride as an arterial sedative, and this paper concludes the labours of the committee. We give the language of the writers as nearly as possible, although at the risk of repetition, as we desire to place before the medical public *all* the evidence we have collected.

Through one of the committee we are enabled to present the accompanying illustrations: The plant, Fig. 1; the root, Fig. 2; the flower, Fig. 3; the seed envelop, Fig. 4; and the early appearance of the plant, Fig. 5; drawn from nature, by Mr. S. W. Abbott, student of medicine. It is hoped that they will aid the country physician in the labour of collecting the same for his own use.

In the present paper we have not arranged the evidence as before, into general and special, but have concluded to summon each witness singly, and suffer him to tell his story to suit himself.

Dr. S. S. GIFFORD, M. M. S. (Member of the Massachusetts Medical Society), says: "I cannot practise without that medicine. I sent to Boston, bought two ounces, paid seventy-five cents. It is good for nothing compared with yours."

Dr. W. W. COMSTOCK, M. M. S., of Middleboro, says: "I have been using the veratrum viride, and have found it in all cases of increased frequency of the pulse, and tonicity, all that its friends have claimed. In atonic diseases, as erysipelas and scarlet fever, I have seen no benefit resulting from its use; but, on the contrary, I am quite sure I have seen it do harm. Its most decided beneficial effects I have witnessed in pneumonia; and, indeed, I should now hardly feel prepared to treat a severe case without it."

Dr. A. RUPPANER, M. M. S., of Boston, reports good effects of the veratrum viride in erysipelas, phthisis, pertussis, and scarlatina.

"CASE I. L. W., a German, with erysipelas of the face. Had been treated by an irregular practitioner. When I first saw him he was delirious, had high fever, pulse 123, very full. Gave gtt. No. v. of your preparation of the veratrum viride, in water, and ordered the same to be given every three hours. Fourteen hours after the first dose, the pulse was reduced to 88. It was constantly kept at that standard, and even lower. Whenever I omitted the medicine, the pulse rose, and fell again as I resumed the same. Patient recovered.

"CASE II. A merchant, aged twenty-two years, with advanced phthisis. Had had repeated attacks of hæmoptysis. Pulse 118—120 per minute. With five drops of the tincture of veratrum viride, thrice daily, the pulse was reduced to 85 per minute, and kept there steadily day after day. Continues the veratrum daily.

"CASE III. A little girl, aged one and a half year, had hooping-cough, followed by pneumonitis. One drop of the tincture of the veratrum viride, given every four hours, succeeded admirably.

"CASE IV. An infant, of eleven months, had scarlatina. During the febrile stage, one drop of the tincture of the veratrum viride, with syrup of squills, four times daily, reduced the pulse readily."

Fig. 1.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 2.

VERATRIA VIRIDE.

Fig. 1, the plant; Fig. 2, the root; Fig. 3, the flower; Fig. 4, the seed envelop; Fig. 5, the early appearance of the plant.

Dr. WILLIAM WORKMAN, M. M. S., of Worcester, has used the veratrum viride for several years. He says: "My observation of the therapeutical effects of the veratrum viride, so far as it is of any value, coincides with the report of your District Medical Society in regard to it. I believe the veratrum viride, when its virtues are fully known, will prove a most valuable accession to our materia medica.

"I think I have seen most decidedly good effects from its use in cases of pneumonia and broncho-pneumonia, where these local inflammations succeeded simple or typhoid fever, and the pulse continued to be accelerated long after the fever had subsided, with harassing cough. It has seemed to me more efficacious than any other remedy I have used, in reducing the frequency of the pulse, and allaying the cough and irritability of the vascular and respiratory systems. I have seen several recoveries under the use of the veratrum viride, combined to be sure, but in cases in which I had great apprehensions would terminate in phthisis. I gave it to a patient with anæmia, for some time, but in ordinary doses its action on the heart was very slight. An indefinite quantity, however, taken carelessly, reduced the pulse for a time from 112 beats per minute to 60. It produced other distressing symptoms, vomiting, burning and oppression in the stomach, faintness and great prostration of strength. The pulse, however, the next day returned to its former frequency, and no perceptible benefit was derived from its use in this case. In some cases of tuberculosis I have given it with apparent benefit."

Dr. Workman gives an example of the collateral use of the veratrum viride. He says: "I first used it several years ago as an external application in a number of cases of obstinate prurigo, which had resisted various other remedies. It was used in the form of an ointment—the pulverized root being combined with sulphur and lard, and applied freely. It produced considerable irritation of the skin—severe burning and smarting, &c.—but was completely successful in removing the disease."

Dr. H. C. BICKFORD, M. M. S., of Billerica, says: "I have used the tincture of the veratrum viride for the last two years. The results, in my hands, have been more marked and satisfactory in pulmonary complaints than in any other. In bronchitis and pneumonia of children, I rely upon it almost entirely. I have realized all the good effects from the medicine which its most sanguine friends claim for it."

Dr. JAMES W. ROBBINS, M. M. S., formerly of Uxbridge, Massachusetts, now of Houghton, Michigan, writes that he made a trial of the veratrum viride and distributed several phials of the tincture to physicians for trial some fifteen years ago. He took it himself and reduced his pulse to 38 beats per minute, keeping at the same time about his ordinary business and experiencing no inconvenience except a little dazzling of the eyes. He says: "It has qualities which will always retain it in use—qualities which no other article possesses, and those of a valuable character. I have watched

its effects more particularly in pneumonia than in any other disease, and here I find it to approach, if not equal, antimony in controlling febrile action without irritating, like that, the alimentary canal. It may be used to great advantage in acute rheumatism, not merely in that case, controlling febrile action, but exerting in a high degree the peculiar influence that other acrid narcotics possess over that kind of inflammation.

Dr. CHARLES H. ALLEN, M. M. S., of Cambridgeport, has used it in nearly all inflammatory diseases, and has observed no ill effects from its use even in infancy. He says: "In all cases I order small doses much diluted, every hour until the arterial action has very much lessened, and for three reasons: 1st, it is more willingly taken, especially by children; 2d, less danger of overdosing exists; and 3d, very much diluted it is more readily absorbed and more quickly effectual."

Dr. E. A. HOLMAN, M. M. S., of Harvard: "My apology for not having reported earlier, the results of my limited observation of the action of this most potent remedial agent is, that the scepticism with which I am largely favored, with regard to medicine, as well as many of the other novelties offered in these fast times, required the confirmation of many trials of the medicine to establish my first impressions, and to enable me to judge accurately of its merits. And now, I unhesitatingly and emphatically declare the veratrum viride to be the most reliable arterial sedative I have used. The diseases in which I have most frequently administered it are typhoid fever, pneumonia, scarlatina and rheumatism, and *not in a single instance have I failed to control the pulse to my entire satisfaction*, thus enabling me to save some patients whom *a thirty years' experience assures me I should otherwise have lost.* I have administered the tinct. veratri viridis in doses varying from one to eight drops at intervals of two to eight hours. In highly inflammatory cases I commence with a large dose, six to eight drops, repeating it every two hours till vomiting ensues, which is generally from the second or third and frequently the first dose, and the pulse falls from 100 or 120 to 50 or 60 beats per minute, and is kept at any desirable point, by two to four drops at intervals of two to four hours. Vomiting, or even nausea, I think unnecessary to insure the sedative effect, and should generally be avoided—in asthenic cases always—though that point should be approximated as nearly as convenient. I have found three or four drops at intervals of three or four hours, ordinarily, as much as the stomach will tolerate. I have not found the veratrum viride cumulative—yet it seems not to lose its power by continued use. The only bad effects I have witnessed, were, the ordinary results of nausea and vomiting, which soon subside on the temporary discontinuance of the medicine, or on the administration of a slight opiate."

Dr. ALEXANDER POOLE, M. M. S., of Chelsea, writes that he has had but little experience with it, but so far as his observation extends, it has acted speedily, and with more certainty than any other article with which

he is acquainted. In one case of pneumonia he gave six drops once in five hours, and by the third dose the pulse had fallen from 120 to 56, at which point he was able to keep it by continuing the dose at longer intervals. He says: "I consider it a very valuable medicine."

Dr. HENRY C. PERKINS, M. M. S., of Newburyport, used the veratrum viride in a case of acute pleurisy after other active treatment had reduced the most acute symptoms, and the pulse fell within three or four days under doses of four or five drops repeated every four or five hours, from 120 to 80 beats per minute. In an aged lady, suffering under acute pneumonia, five drops of the tincture of veratrum viride every three hours seemed to produce diarrhœa. Dr. Perkins also states, that his friend Dr. Howe, had a case of pneumonia in a child of ten years, where the sedative effect of the tinct. veratrum viride was most marked. "A similar remark I might make (second hand) from Dr. Merrill, of Hampton, N. H. I have no doubt myself of its similarity to digitalis, and from what I have seen and heard of it, should believe it a much safer medicine to be used in dyspnœa, inflammatory affections, etc."

Dr. OTIS E. HUNT, M. M. S., of Weston, regards it as a valuable medicine. His manner of using it has been to give five drops of the tincture every thirty minutes till the pulse fell. He says a few drops would sometimes accomplish the object, and at other times a half dozen would be required. He has found it very apt to produce nausea.

Dr. JOHNSON GARDNER, M. M. S., of Pawtucket, says he has frequently administered the veratrum viride to his patients, where an arterial sedative was indicated, and has found it a far more potent and certain agent than he had previously supposed.

Dr. C. W. WHITCOMB, M. M. S., of Barre, speaks favourably of the veratrum viride as an arterial sedative, though not *always* reliable, but as much and more so than any other remedy with which he is acquainted.

Dr. FRANCIS MINOT, M. M. S., of Boston, reports the following case:—

A young man, nineteen years of age, entered the Massachusetts General Hospital under my care, Oct. 9th, 1858, with acute pericarditis. His pulse was quite irregular in frequency, varying between 96 and 116. On the 12th, at the morning visit, it was at 112 to 116 per minute. The inspirations were 30 per minute. He was ordered four drops of Thayer's fluid extract of veratrum viride (which is the preparation commonly used at the Hospital) once in two hours. He became more comfortable during the day. The pulse in the evening was at 96. He had a more quiet night.

Oct. 13th. Pulse 88; respiration 36. Reports that he is better. He took seven doses of the veratrum, and it was stopped this morning on account of the patient's vomiting.

14th. Comfortable night. Pulse 112; respiration 36. Resume veratrum.

15th. Took six doses of the veratrum without vomiting and without much effect on the pulse, which is now 112; respiration 30. Increase veratrum viride to gtt. No. v.

16th. Took three doses of veratrum yesterday. Pulse in evening 110; now 104; respiration 40. Take veratrum only when feverish.

17*th*. Pulse 112; respiration 24. (Pleurisy of left side.) Veratrum gtt. v, every four hours.

18*th*. Yesterday he was tranquil. Pulse in evening 108; now 96; respiration 22. Took five doses of veratrum, and vomited after the last.

19*th*. Patient appears better. Pulse 96; respiration 24. Took four doses of veratrum, nauseated by the last.

20*th*. Pulse 100; respiration 30. Took three doses of veratrum, and was nauseated by it.

21*st*. Pulse 96; respiration 28. Signs of effusion into left chest. Took four doses.

22*d*. Pulse 96. After examination of chest 120. Congestion of right lung. Took five doses of the veratrum.

23*d*. Pulse 100.

24*th*. Pulse 104. Veratrum gtt. iij, every three hours.

25*th*. Pulse 100.

27*th*. Pulse on waking from sleep 120. Omit veratrum.

The cardiac symptoms returned at this time. Soon after he came under the care of Dr. Bowditch. He had œdema of the legs and other grave symptoms.

Nov. 20*th*. Had an attack of pain and palpitation after dinner yesterday. Took tinct. veratri viridis gtt. ij, with a drachm of solution of morphia with much relief. Asks to have the veratrum continued. Resume it gtt. vj, thrice daily.

21*st*. Better. Vomited twice after taking veratrum. Pulse 100.

23*d*. Better. Only two half doses of the veratrum borne. Pulse 100.

26*th*. Improving. Pulse 112. Omit veratrum. This patient finally recovered. The remedy appeared to have considerable effect in diminishing the febrile condition of the patient, but did not prevent the complications of pleurisy and pulmonary congestion.

Dr. M. R. RANDALL, M. M. S., of Rehoboth, says that he is compelled to add his testimony in favour of the veratrum viride, as one of the most valuable medicines as an arterial *regulator* that has ever come under his notice. In concluding his letter, he says, "I would not be deprived of it on any consideration whatever."

Dr. A. A. KENDALL, M. M. S., of Newton Lower Falls, says: "I have used the veratrum viride constantly in my practice for more than two years, and with ever-increasing satisfaction. I have found it very reliable as an arterial sedative in acute rheumatism, scarlatina, pneumonia, pleurisy—in short, in all cases attended with *sthenic* arterial excitement. I have given it to patients of all ages, and very rarely have I seen any unpleasant effects—in but one instance has there been distressing nausea and vomiting. Sometimes, though rarely, I have seen no appreciable effect upon the pulse."

Dr. C. A. WILCOX, M. M. S., of Uxbridge, says: "I have made frequent use of the veratrum viride during the last twenty months, having prescribed it in pneumonia, pleuritis, peritonitis, and acute rheumatism, also in hæmoptysis from consumption, when attended with a frequent pulse. From this repeated use I have come to look upon it as a valuable remedy,

almost certain in its effect as an arterial sedative, and through this it becomes an antiphlogistic which I think does not depress the vital forces like bleeding, antimony, and active purgatives. In the case of a female, 22 years of age, from a strongly marked consumptive family—reduced by lactation, seized with double pneumonia, her general condition forbidding bloodletting—the state of the bowels not admitting of cathartics—stomach refusing nauseants—and the patient growing worse daily. I commenced with the veratrum viride on the fourth day of treatment, with the pulse at 135 beats per minute. The next day it stood at 120, the next after that at 100, and on the third day at 85. All the other symptoms improved in proportion with the pulse. I have thought it acted as an expectorant, but have considered this, as well as most other properties ascribed to it, as depending upon a reduced inflammatory action which most surely follows its sedative effect."

Dr. JOSEPH G. S. HITCHCOCK, M. M. S., of Foxborough, has used the veratrum viride several years. Thinks it must satisfy all *reasonable* expectations. Thinks the tincture made from Southern roots better, probably, than that from plants growing at the North. Thinks our tincture not equal to that he had been using, and not as certain in its effects. Ranks the veratrum viride along-side of antimony.

Dr. JAMES M. NYE, M. M. S., of Lynn, has used the veratrum viride in many cases, and with great benefit, particularly in puerperal fever, peritonitis, pneumonia, pleuritis, and also in other diseases when attended with great excitement of the heart and arteries. In his practice, he says it has not failed to control the pulse when given in the ordinary doses. Thinks it especially useful in pneumonia and pleurisy.

Dr. A. D. BACON, M. M. S., of Sharon, says he has never found any other drug so sure and so efficient as an arterial sedative. He has used it for two or three years, and it has seldom failed of producing the desired effect upon the pulse. Has found it especially useful in cases where there has been a tendency to congestion of the lungs. In such cases he says he should rather be without any other article of the materia medica than this; can call to mind cases where he thinks he should have lost his patients but for the veratrum viride. "I have just discharged two patients, one an old and the other a young man, in whom it seemed to act like a charm, speedily reducing the pulse from 100 or more, to 70 in one case, and to 60 in the other, thus relieving the dyspnœa, removing the congestion and at once diminishing or suppressing the bloody expectoration, and thus permitting the vital current to flow on without obstruction in its wonted channels."

Dr. F. F. FORSAITH, M. M. S., of South Abington, had used the veratrum viride some months previous to receiving our tincture. In many instances he has been exceedingly pleased; in some disappointed. "In many of these latter cases, had I been so situated as to be able to admin-

ister the medicine myself or to watch it closely, so as to obtain its full effects, I have no doubt it would have been otherwise. In using an agent of so much power for good or for evil, I think too much caution cannot be enforced. Still, I have used it almost constantly for nearly two years, and have been so much gratified with its effects when pushed to its proper extent, that, in all cases suited to its exhibition, I rely upon it; finding it to act with more efficiency as an arterial sedative, and with less disturbance of the system generally than any agent with which I am acquainted. I send you the report of two cases, one of which is of an ordinary type, and the other the only instance in which I have observed any really serious difficulty from its use.

CASE I. *April* 25, 1859. W. S., aged two years, had been under domestic treatment for three days. Pulse 150; tongue heavily coated; skin hot and dry; no appetite; a dry cough; rough respiration over most of right lung: some delirium. Tincture of veratrum viride two drops once in three hours.

26*th*. Pulse 128; tongue still coated heavily; cough rather looser; skin losing its dry, harsh appearance. Continue treatment.

27*th*. Pulse 92; tongue cleaning; cough loose and frequent; skin moist. Continue veratrum and give ol. ricini f℥iss.

28*th*. Pulse 92; tongue cleaning finely; some appetite.

30*th*. Pulse 88; appetite fair; sleep quiet; cough loose; tongue nearly clean; patient sitting up.

CASE II. *Jan.* 19, 1859. F. E., 2½ years of age. A delicate child. Has been ill for two or three days. Restless and irritable. No appetite. Tonsils much inflamed, particularly the right. Inflammation extending somewhat upon the lungs. Pulse 120; tongue thickly coated. Tincture of veratrum viride, gtt. ij, to be given once in three hours.

20*th*. 9 A. M. Pulse the same; increased dyspnœa; skin hot and dry. Tr. veratri viridis gtt. iij, to be given once in three hours. The pulse became reduced gradually, the skin more moist, and the symptoms generally more favourable. At 9 P. M. I was hastily summoned, as they "feared the child was dying, as there was spasmodic action of the limbs, and the eyes were set in her head." Found the statement not exaggerated. Pulse 40, wavering and uncertain; respiration 15, laboured and sighing. Ascertained that the mother had administered the medicine a little before the stated time, and the father, not knowing it, had given an extra dose a few minutes after. With alcoholic stimulants and external irritations the circulation by degrees grew natural, and before morning was much the same as on the day previous. The veratrum viride was resumed in smaller doses and with more caution, and all went on well till entire recovery in about one week.

Dr. E. W. A. HARLOW, M. M. S., 52 Essex Street, Boston, sends us an unfavourable report of the effects of the veratrum viride as an arterial sedative. We are much obliged to him, as we wish to give a truthful representation of the whole subject, and not to claim more than its merits deserve. He says:—

"I have used the tincture of the veratrum viride, which I received a year ago, in two cases. The first was that of a girl, 15 years of age, with

rheumatic carditis. Her pulse was 120 or more day after day. I directed eight drops of the tincture of veratrum viride three or four times a day. The medicine was taken for about four days in succession, but without any appreciable effect upon the circulation. Certainly the pulse was not diminished in frequency."

Perhaps Dr. Harlow might have succeeded better by giving the same dose every two hours till nausea or vomiting supervened, and then continued the medicine in smaller doses.

"The second case was that of a young man, 23 years of age, affected with hæmoptysis. Soon after each attack of hemorrhage the pulse became rapid, from 120 to 140 beats per minute, full and hard. He had what is sometimes called a 'hemorrhagic pulse.' He took the tincture of the veratrum viride every three hours, commencing with three drops, and increasing each dose one drop. I could not perceive that any effect was produced upon the pulse until vomiting occurred. Soon after vomiting the pulse would begin to resume its great rapidity, when the veratrum viride was again administered, commencing with three drops and increasing each dose as before."

Dr. GEORGE D. COLONY, M. M. S., of Athol Depot, reports that he had used Tilden's fluid extract of veratrum viride previous to the reception of our tincture, and finds them apparently of the same strength. In all of those cases in which the medicine was fairly tested, its remarkable sedative properties manifested themselves in a very great reduction of the force and frequency of the pulse, perspiration, and as great an alleviation of all the other symptoms as usually follows venesection. Dr. Colony gives the interesting report of two cases that occurred under his observation. He says:—

"CASE I. About a year since I was in attendance upon a gentleman, F. T., about 40 years of age, sick with pneumonia. The patient was predisposed to phthisis, and had for some time been in poor health. With other marked symptoms of this disease the pulse was at 125. I at once resorted to the veratrum viride, without the aid of any other important remedy except cupping. I commenced with four drops of Tilden's preparation and increased to six, repeating the dose once in four hours. I was suddenly summoned in the night, not more than twenty-four hours after the beginning of this treatment, to see the patient, who, with his attendants, were much alarmed at the change which they had observed. I found my patient, as I anticipated, in reality much improved; for though there had been some retching and ineffectual attempts at vomiting, and the man thought himself dying, there was no ground whatever for alarm. The pulse had fallen to 75 or 80 beats in a minute, and the respiration was improved. The patient could not be induced to follow the treatment, and the pulse rose to nearly 100; but the severity of the attack was materially abated from that time, and the patient recovered his usual health.

"CASE II. A young Irishman, of good constitution, was sick with pneumonia. Pulse 120, with strong febrile excitement. I commenced as I had been in the habit of doing, with four drop doses; but, either from a want of faithfulness in the administration or other cause, no result followed after

twenty-four hours. The dose was increased to six and seven drops, and strict regularity enjoined. But two or three doses had been taken before I was anxiously informed that 'Johnny was dying.' I was not alarmed, however, and found nearly the same result had taken place as in the case above related, and the patient speedily recovered."

Dr. ENOS HOYT, M. M. S., of Framingham, writes that he has used the veratrum viride in the apoplexy of aged people and those of enfeebled health, and finds it better than bloodletting, as the recovery is better. Has given as high doses as twenty to forty drops in four cases that came under his care, which resulted in recovery—which was heroic treatment. He thinks he has derived much benefit from its use in scarlatina, and he learned while at the South that the physicians there were using it with good results in the same disease. Where arterial excitement needs reducing it is decidedly better than digitalis. He says he likes it in measles, pertussis, and parotidea. He is testing it in a case of scirrhous breast, and thus far it has certainly arrested the increase of the tumour. He has also given it with good results in acute rheumatism. As collateral uses of the veratrum viride he has found it in the form of a bath or an ointment give great relief when applied to swelled and painful joints. In this connection one of the committee would state a similar experience in the external use of the veratrum viride. It has been employed in simple and gonorrhœal rheumatism, in mastitis, in puerperal peritonitis, and in erythema.

Dr. G. L. SIMMONS, M. M. S., of Sacramento City, Cal., has used the veratrum viride constantly for about two years, at the Sacramento County Hospital and in private practice, and has rarely been disappointed in its action. He has employed it in pneumonia, continued fever, acute rheumatism, and palpitation of the heart. He has employed it in scarlet fever combined with the chlorate of potash, and has frequently added it to cough mixtures for bronchial affections. He says he should be very unwilling to give up the use of the veratrum viride, as he regards it as one of the most potent and reliable articles of the materia medica.

Besides the above evidence derived from members of the Massachusetts Medical Society, the committee have received testimonials from other regular practitioners of medicine who have employed the tincture of veratrum viride, and who are not fellows of the above society.

Dr. A. B. CROSBY, of Hanover, N. H., thus writes:—

"It gives me great pleasure to testify to the admirable properties of the veratrum viride as prepared by you. I have found it much more reliable than Tilden's extract, and decidedly the most manageable arterial sedative I have ever used. I have witnessed no ill effects from its exhibition, but have found it an obedient and reliable servant. I have employed it during the prevalence of typhoid fever with marked good effect. In quite a number of instances where the symptoms of incipient typhoid fever were present its use has been followed by the immediate reduction of the pulse thirty or more in the minute, and the convalescence of the patient in from four to

seven days. In affections of the lungs it has seemed to answer an admirable purpose, reducing the frequency of the heart's action so rapidly as to make bloodletting unnecessary. After surgical operations *involving the peritoneum* I have been well satisfied with its results. My father, Prof. Dixi Crosby, has employed it under similar circumstances, and I am permitted to say that he regards it as one of our most valuable agents in controlling the action of the heart. I cannot doubt that the veratrum viride will vindicate the high position which it now occupies, and will continue to be one of the most prominent and valuable articles in the materia medica."

Prof. E. R. PEASLEE, eminent as a teacher, a physician, and surgeon, remarked to one of the committee that he regarded the veratrum viride as being invaluable as an arterial sedative, and that he had derived the happiest results from its use.

Dr. C. S. BISHOP, of Philadelphia, Pa., has tested the tincture of the veratrum viride, prepared by ourselves, in many cases, a few of which he reports. He has particularly employed it in surgical practice with satisfactory results. In the irritation succeeding burns and operations it has proved a valuable sedative.

CASE I. *Medical student, injured by fire-works. Pulse kept at* 60. *The veratrum quelling the pain.* "One of the first patients to whom I gave it was a medical student, from Texas, who was injured by the explosion of a piece of fire-works—being burned about the face and neck. Saw him at about 10 o'clock P. M., two hours after the accident occurred. Applied glycerine and lime-water to the burned surface, and gave eight drops of the tincture of veratrum viride every two hours. At nine o'clock on the next morning his pulse, which at first was 100, had fallen to 51 beats per minute; and he told me that when he was lying perfectly still it had been as low as 48. He had taken forty drops of the medicine—having omitted two doses on account of the distressing nausea which it produced. He continued to take just enough of the veratrum viride to keep his pulse at about 60 beats per minute, for thirty-six hours, by the end of which time that portion of the burn which was merely reddened, had resumed its natural colour, and that which was vesicated was pale and healing rapidly. *From the time that the sedative influence of the veratrum viride became established he felt no pain.*"

CASE II. *Anchylosis of elbow.* "A boy, of about ten years of age, was brought to me with an injury of one of his elbows of some four weeks' standing. It proved on examination to be a fracture of the internal condyle, involving the trochlea, and was attended with the usual dislocation. Anchylosis had taken place to such a degree as to prevent motion, unless considerable force was applied. While he was under the influence of chloroform I broke up the adhesions, and reduced both the fracture and the dislocation. An anterior angular splint was applied, and the joint kept wet with cold water. Two drops of the tincture of veratrum viride were given every two hours, and the pulse was kept at between 60 and 70 beats per minute for about one week. There was little or no pain at the seat of injury, and very little heat or swelling. In about the usual time in such cases he recovered *perfectly* the use of the joint."

CASE III. *Contusions and cuts.* "A man, of about forty-five years of age, was very extensively cut and bruised by having been dragged some

distance over the cobble-stones, beneath a wagon, which had upset upon him, with a runaway horse attached. Applied cold water to the injuries, and gave him six drops of the tincture of veratrum viride every three hours. His pulse, which counted 90 one hour after the accident, was kept at 58 to 60 beats per minute for four days, and it was exceedingly gratifying to notice how rapidly the bruises disappeared and how kindly the lacerations healed."

"In two cases of lacerated wounds of the scalp I had the happiest effects to follow the use of the veratrum viride. In fractures I ordinarily give it four or five days, and find that there is much less pain and swelling at the seat of injury than is ordinarily seen in those cases—the fractures uniting just as readily, and the patient's comfort greatly increased thereby. After surgical operations, where I wish to avoid inflammation and promote union by the first intention, I ordinarily resort to the veratrum viride."

The following case is similar to some that have come under the knowledge of the committee, which may be cited as cases of overdosing or poisoning—but, *however severe and alarming the symptoms, no well-authenticated cases of death have occurred;* on the contrary, it has happened that the overaction has proved a crisis in the complaint, and the patient rapidly recovered from that time. If the reader has observed any *fatal case* of poisoning by the veratrum viride, the committee would be much obliged to him to furnish an account of the same.

"CASE IV. A little girl, about four years of age, labouring under convulsions from congestion of the brain. Pulse 120. Applied cold to the head, and gave one and one-half drops of the tincture of veratrum viride every hour. Three doses were given when she broke out into a profuse cold perspiration, and all the symptoms of prostration. Her friends thought her dying, and sent for me. I did not arrive there until two hours after I was sent for, that is, in about four hours and a half from the time she was first seen. Instead of finding her dead, she was lying upon the bed, playing with a doll, *perfectly relieved,* her pulse at 72 beats per minute."

"CASE V. In a case of pleuro-pneumonia, in a woman of about thirty years, I gave eight drops every two hours. After the fourth dose was taken, the patient fell asleep, and did not wake for six hours. Her pulse, which was at 95 beats per minute when she took the first dose, had fallen to 65 the next morning, and the pain had very much abated. In four days she was able to be about, having had her pulse kept at between 60 and 70 beats per minute during that time. At my second visit it was ascertained that the patient had not taken her medicine since she awaked, for the simple reason that her little girl (two and a half years of age) had gotten hold of the cup, in which it was contained, and drank its contents all up. I was startled; the child had swallowed *thirty-two drops of the tincture of veratrum viride at one dose.* On looking to the opposite side of the room, where she was busily engaged with her playthings, I remarked that I presumed she had not kept it down a great while; the reply was, that 'it was no sooner down than up again.'"

Dr. Bishop has also derived excellent effects from the veratrum viride in

pleurisy, pneumonia, scarlet and remittent fevers, measles, asthma, convulsions, ovaritis, metritis, and caked breasts, and in fact in most cases where a sedative and relaxant was required. He is satisfied that with proper precautions against its prostrating effects it may be safely used, and that it is a very valuable addition to our list of arterial sedatives and antiphlogistic remedies.

The committee would tender their thanks to the gentlemen who have reported their experience as given above. They only regret that more did not respond to their call, and if the results, obtained by those who have kept silence, do not accord with the published record, they hope that such will report their differences without delay. But the time has most surely gone by when the value of the veratrum viride, as a medicinal agent, is to be regarded as hypothetical. It has been used by thousands of physicians fully competent to make observations with discrimination and sound judgment. The strongest and most conclusive testimony relative to its great value comes from those who have used it the most, and who are, therefore, the best prepared to express an opinion.

Some of them feel as though they could not get along without it, especially in view of the fact that with it they have saved patients whom they think they must have lost without it. But there are others who will not believe in its value, and make a fair trial of it. They either sneer at it, or pretend to be afraid of so potent an agent. And yet, among all the cases that have been recorded throughout this whole country, can a single instance be mentioned where serious harm has been wrought by its use? Of which of our other potent remedies can this be said? The failure or success of this or any other medicine, in a single case, proves nothing for or against it. It is the result of repeated trials that is to establish its value and certainty of action. Nevertheless there are men who, having used it with trepidation and misgivings in one or two cases, without seeing the effects ascribed to it, have thrown it aside, and pronounced it worthless. But it matters little who now remain sceptical in respect to the great value of the veratrum viride as a medical agent. It has been used extensively, and has vindicated the reputation given it, to the satisfaction and admiration of intelligent men all over the country, and supplied a want that nothing hitherto has done.

It seems not inappropriate in closing this paper to make mention of the fact that the thanks of our medical brethren have been freely and cordially bestowed upon this District Society, for the noble efforts—as they are pleased to call them—that we have made to bring what is now proved to be an exceedingly valuable remedy, properly before the profession. Let us accept these expressions alike honourable to those who bestow them and to us upon whom they are bestowed. May the consciousness that our efforts to benefit our noble profession have been appreciated and acknowledged, excite in us the honourable ambition of doing what we can to alle-

viate the sufferings and to restore to health those who may be called to beds of pain and languishing. *Would it not be pleasant to each of us to know, while toiling amid our daily cares and labours, that our names would be remembered with gratitude and honour for the good we had done to our race, while our bodies are quietly resting beneath the green turf of the valley?*

Addendum.—The closing phrase of this paper may be read with more interest when it is known that the remains of the writer, Dr. Truman Rickard, now sleep beneath the "turf of the valley" of which he spoke. He died August 8th, 1861, aged forty-seven years, a Christian, a scholar, and good citizen.

WOBURN, August, 1861.

ART. VI.—*On the Detection of Strychnia as a Poison, and the Influence of Morphia in disguising the usual Colour-test.* By JOHN J. REESE, M. D., of Philadelphia.

THE progressive increase in the number of deaths, within the last few years, occasioned by strychnia, used either for homicidal or suicidal purposes, is a subject demanding the careful consideration both of the toxicologist and the medical jurist; and every circumstance connected with the detection of this most potent agent cannot fail to interest the medical profession at large.

I have lately had occasion to investigate this subject very closely, in connection with a case of alleged poisoning by strychnia. A man was indicted for the murder of his wife, before the Court of Oyer and Terminer of Perry Co., Pa., at the April term of the present year. The woman died in December, 1860. Although the circumstances of her death were such as to excite suspicion at the time, no examination of the body was made until six weeks had elapsed, when the feelings of the community appear to have become sufficiently aroused to demand an investigation. The body was accordingly disinterred, and the examination was conducted by several respectable physicians of the place. They found the body still in a good state of preservation, but very rigid. The lungs were deeply engorged with dark blood, which escaped very copiously when they were cut into. The heart was healthy (although it was attempted to prove subsequently in the defence, that the woman was subject to cardiac disease); it was not empty and contracted, as has sometimes been found under similar circumstances, but it contained a considerable quantity of blood. The brain and spinal marrow were not examined, which is to be regretted, inasmuch as these organs are almost uniformly found congested in cases of death

resulting from strychnia. The stomach and a portion of the small intestine were carefully tied, and along with the adhering pancreas, were reserved for a chemical analysis. The removed viscera were brought to Philadelphia, and I was requested to undertake the examination, which I commenced just eight weeks after the death of the subject.

I found the stomach but comparatively little changed in appearance, considering the length of time elapsed since death. Its lining membrane was healthy, with the exception of a few dark congested spots. There was neither ulceration, softening, nor erosion of the membrane. The same is true also of the fragment of intestine examined. The contents of the stomach and bowel consisted of about four or five ounces of a thick, brownish, homogeneous fluid.

The chemical examination was conducted with scrupulous care. I made *three* separate analyses—one of the contents of the stomach, one of the contents of the intestine, and a third of the tissues themselves; and each one of these was repeated a second time, so as to avoid all possible error. Yet I entirely failed to detect any evidence of the presence of strychnia, either by the bitter taste of the final extract, or by the very delicate *colour-test* employed.

Now, inasmuch as all the *moral* circumstances connected with this case (as elicited in the subsequent trial), as well as the symptoms attending the woman's death, clearly pointed to poisoning by strychnia, I felt desirous of ascertaining if there was any adequate cause which might satisfactorily explain the failure to detect the poison. In the first place, as is very well known, it is, as a general rule, more difficult to detect the presence of an *organic* poison in the stomach than of a mineral poison. For example, arsenic, antimony, or mercury may almost always be discovered with absolute certainty, even long after death, provided the individual had not survived long enough after taking the poison to allow of its total elimination from the system—a period averaging about two weeks. This is not the case, however, with an *organic* poison, either vegetable or animal. The alkaloids, especially, are known to undergo very rapid absorption from the stomach, and elimination from the blood. In their passage through the system, it is quite reasonable to suppose, from their complex nature, that they follow the general law of chemical decomposition; and that, unless searched for very soon after administration, the likelihood of detecting them would be extremely small. Now the case before us was just of such a character. The individual survived five or six hours after swallowing the alleged poison—an unusual length of time; again, a period of eight weeks elapsed after death before the chemical examination was undertaken. And we have the highest authorities for believing that organic poisons do undergo change in the body, *after* death.

There was, however, one other circumstance connected with the case, which to my own mind was of especial interest and importance—namely,

the fact that the woman had taken, just before death, by the advice of her medical attendant, *a quarter of a grain of morphia* with a little ipecacuanha; but that she did not vomit. Now, the value of this fact is just this: it has been ascertained that the presence of morphia and other substances has the effect of disguising, and entirely neutralizing the usual *colour-test* used for detecting strychnia; so that, although the latter might be undoubtedly present, yet, if morphia were also present at the same time, the strychnia could not be discovered. It will be readily admitted that this is a point of the extremest importance to be settled by the chemist, in medico-legal researches. It is one to which no very especial attention has hitherto been given. It is merely mentioned as a casual fact in the various works on toxicology, but the only actual experiments recorded, to my knowledge, are those published by Dr. T. G. Wormley, in the *Ohio Med. and Surg. Journal*, for September, 1859, in which it is stated that, when the morphia *exceeds* the strychnia in quantity, the possibility of discovering the latter by the colour-test diminishes. The author himself lays no particular stress upon the fact. I accordingly undertook a series of experiments for the purpose of more accurately determining, if possible, this very important subject, and with the following results:—

Experiment 1. One-tenth of a grain of pure strychnia was added to about twelve ounces of water, into which were put several ounces of fresh beef, finely cut up, together with some starch, a little common salt, and a few drops of acetic acid; (the object being to represent, as closely as possible, the contents of a human stomach after a meal.) The whole was digested on a sand-bath for 12 hours at a moderate heat. It was then strained, pressed and filtered; and afterwards evaporated down to a very small bulk. It was next divided into two separate portions, each of which, of course, would contain the $\frac{1}{20}$th of a grain of strychnia. One of these portions was treated after the process known as Graham and Hoffman's (the alkaloid being removed by animal charcoal, and finally extracted by ether). Here, a drop or two of the ethereal solution, representing about the 1-40,000th to the 1-50,000th of a grain, gave distinct evidence of strychnia by the usual colour-test. The second portion of the evaporated solution was divided into two parts, each of which would of course contain the $\frac{1}{40}$th of a grain of the alkaloid. The first of these was treated according to the process of M. Staäs, in which ether is used as the ultimate solvent: and the second part after the process of Mr. Prollius, in which the ultimate solvent was chloroform. In both instances I obtained the most satisfactory proofs of the presence of strychnia; operating upon a single drop of the fluids—which would represent, certainly, not over the 1-100,000th of a grain of strychnia.

Expt. 2. This was a repetition of the former experiment, except that the quantity of strychnia used was much smaller—only the $\frac{1}{160}$th of a grain. After treatment by Staäs' process, and on concentrating the ultimate ethereal solution, the presence of strychnia was manifested both by the colour-test, and by the bitter taste of the extract. Here, the quantity of the poison operated upon was less than the 1-100,000th of a grain.

Expt. 3. This was an exact repetition of Expt. 2, except that to the $\frac{1}{160}$th of a grain of strychnia, three times that quantity of morphia ($\frac{3}{160}$d of a grain) was added. On treating this by Staäs' process, as in the preceding cases, I could not discover the slightest trace of either strychnia or morphia, even after the ultimate ethereal solution was concentrated to a very small bulk by evaporation.

Now, from the first two experiments just mentioned, in which the processes used were the precise counterparts of those employed in the analysis

of the stomach, I think I am justified in concluding that these processes were extremely delicate and reliable: and that if the poison had been present, even in so minute a quantity as the $\frac{1}{100}$th of a grain, it ought to have been discovered, unless its presence had by some means been concealed. The *third* experiment would seem to prove, most unequivocally, that morphia, when present in excess, along with strychnia, has the property of so concealing the latter, as to render it impossible to detect it by the usual chemical test.

Expt. 4. This was also a repetition of Expt. 2, except that to the $\frac{1}{100}$th grain of strychnia, the $\frac{1}{50}$th grain of morphia was added instead of the $\frac{1}{30}$d—or double instead of treble the quantity. Here, likewise, there was a total failure to discover the poison.

Expt. 5. This was a repetition of the last, except that only $\frac{1}{100}$th grain of morphia was added to the $\frac{1}{100}$th grain of strychnia, or an equal portion. The result here was that I obtained the faintest possible evidence of the presence of strychnia, and only after repeated trials.

From these last experiments I think we may conclude that *the influence of Morphia in preventing the detection of minute quantities of Strychnia, in the presence of an oryanic fluid, depends upon the relative quantity of the two alkaloids;—the Strychnia being not discoverable when the Morphia is in excess, and barely discoverable when in equal quantity.*

I next instituted another set of experiments, with a view of ascertaining the effect of morphia in disguising the presence of strychnia *in perfectly pure solutions*—that is, entirely free from all organic mixtures. I purposely made use of extremely minute quantities of these substances, inasmuch as I was desirous of drawing correct inferences, which might be applicable in testing for the smallest quantities of the poison existing in the stomach. The general results of these experiments are interesting, and somewhat curious, as exhibiting the fact, that not only is the difficulty of detecting strychnia greatly increased by increasing *the proportion* of morphia, but that the *actual amount* of strychnia that can be discovered is nearly in an inverse ratio with the amount of morphia used. For instance, when the proportion of the two alkaloids was *one to one*, I was able to detect as minute a quantity as the 1-500,000th of a grain. When the proportion was one of strychnia to two of morphia, the smallest discoverable amount was about the 1-300,000th of a grain. When the proportion was *one to three*, the quantity discoverable was the 1-150,000th of a grain. When *one to four*, the minimum quantity was about the 1-100,000th of a grain. When *one to five*, the minimum quantity was 1-80,000th of a grain. When the proportion was *one to ten*, the smallest quantity discoverable increased to the 1-10,000th of a grain: and when the proportion was as high as *one to twenty*, the quantity rose to the 1-5,000th of a grain. Beyond this I did not pursue my experiments, as I was satisfied as to the general results. Dr. Wormley's observations, as recorded in the paper already quoted, go to show that the detection of strychnia, in the presence of an

excess of morphia, is really more difficult when the amount of both alkaloids is very large, than when it is comparatively small; thus, he states that, when he experimented on one part of strychnia with one-and-a-half of morphia, in a large quantity, "the reaction" of the colour-test "is just perceptible." One part with two of morphia gives "but a mere trace" when the quantity of strychnia is as large as the $\frac{1}{30}$th of a grain. One part with three of morphia gives "no reaction" when the quantity is large.

But the important point was to determine how a quantity, almost infinitesimal, might be affected by an amount of morphia, which, though small in itself, yet bore a relatively large proportion to strychnia; just precisely such a state of things as would be likely to be met with in an analysis of the human stomach.

In connection with the foregoing experiments, and with the view of still more closely testing the question of how far the presence of morphia would operate in disguising strychnia, when both had been present in the stomach of a living animal, I administered the poison to three half-grown cats, with the following results:—

Half a grain of pure strychnia was given to the first animal, and in eleven minutes it died in a violent convulsion. The poison was very easily discovered in its stomach, by the usual tests, on the following day. To the second animal a quarter of a grain of strychnia and the same quantity of morphia were given; and, somewhat to my surprise, the animal was deeply convulsed in six minutes, and died very quickly. Here the morphia, so far from counteracting the toxic influence of strychnia (as might have been inferred from its opposite physiological influence), seemed actually to have increased its effects.[1] The stomach of the second animal was likewise examined, but I obtained scarcely recognizable evidence of strychnia, owing doubtless to the influence of the presence of the associated morphia. It will be recollected that the quantity of morphia in this case was just equal to that of the strychnia. To the third animal, the $\frac{1}{20}$th of

[1] This fact is in accordance with the observation of Bernard, in relation to the effect of the woorara poison not counteracting, but rather increasing the violence of the poisonous action of strychnia, although these two substances, separately, possess such opposite physiological properties—the one being a powerful neurotic excitant, the other an equally powerful depressant. It must be remembered, however, that another high authority, M. Vella (*Comptes Rendus*, t. lx.), makes an opposite assertion to that of Bernard; he contends positively for the antagonism of the two poisons.

The experiments of Drs. Hammond and Mitchell (Experimental Researches on Corroval and Vao, by Wm. A. Hammond, M. D., and S. Weir Mitchell, M. D.; *Am. Journ. of Med. Sci.*, July, 1859), on *corroval* and *vao* (supposed varieties of curara, though differing from it in some physiological points) seem rather to sustain Bernard's views on this point. When either of these substances was administered, along with strychnia, to frogs, they did not neutralize each other, but the peculiar impression of both poisons was produced.

a grain of strychnia, and the $\frac{1}{10}$th of a grain of morphia (double the quantity) were administered. Convulsions took place in about fifteen minutes, and death in half an hour. The stomach was examined by Staäs' process, as in the other cases, but with a total failure to detect the poison by the colour-test; although the bitterness of the extract, and its decided action in producing tetanic convulsions in a number of frogs, clearly established its presence.

From all the foregoing experiments, it appears to be conclusively established that morphia does unquestionably possess the power, when present in excess, of completely disguising the usual colour-test of strychnia; and this is emphatically the case when they are associated in organic mixtures, as in the contents of the stomach. Consequently, this fact should always be taken into the account, in medico-legal investigations.

In making the foregoing statement, I regret to find that I differ from so high an authority as Prof. Guy, of King's College, London, who has communicated a valuable paper "On the Colour-tests for Strychnia, &c.," in the *Chemical News* of July 6, 1861. While discussing the question of the effect of the presence of other bodies on the colour-test, this author states, as the result of his experiments made with a great variety of substances—vegetable, animal, and mineral, and including all the matters that would be likely to remain mixed with strychnia when extracted from the contents of the stomach, or from the fluids and tissues of the body—"that the colour-tests are little, if at all, affected by such admixtures."

Now, I feel assured that Dr. Guy could not have experimented on a mixture of strychnia and morphia, or he would have arrived at a different conclusion *in this one instance.* Indeed, he does not enumerate *morphia* among the substances tried by him in association with strychnia, as it probably did not occur to him to employ such a mixture.

So thoroughly was my own mind convinced upon this point, that I felt conscientiously bound to give it in evidence, on the trial of the prisoner, when I was interrogated as to "what causes might prevent the detection of strychnia in the stomach?" As I have already mentioned, the chemical analysis had entirely failed to establish its presence; nor could I discover the slightest bitterness of taste in the ultimate extract. Nevertheless, the prisoner was convicted of murder, probably on the very strong *moral* evidences of his guilt, and was sentenced to execution in spite of the most persistent assertions of his innocence. I have since learned, however, that he subsequently made a full confession of the crime.

It may not be out of place, in this connection, to refer to the most approved methods of testing for strychnia, and also to indicate their relative value. Among the various chemical tests which have, from time to time, been proposed, the only one to which I shall here allude, as entitled, beyond all comparison, to the pre-eminence, is the *colour-test.* This name has been most appropriately given, on account of the beautiful play, or succes-

sion of colours, produced when certain oxidizing bodies are brought in contact with strychnia, in the presence of sulphuric acid. The substances usually employed for this purpose are the peroxides of lead and manganese, bichromate of potassa, ferricyanide of potassium (red prussiate), and permanganate of potassa. While these agents, in conjunction with sulphuric acid, will yield up oxygen to organic substances generally, the alkaloid Strychnia is the only body known which, under the circumstances just mentioned, will exhibit the beautiful *play of colours* already alluded to. When, for example, a small fragment of strychnia is placed upon a white porcelain plate, and a drop of strong sulphuric acid is added, and then a small portion of any of the oxidizing bodies above enumerated is stirred in with it, a beautiful rich violet-blue colour will be given, which very soon changes to a mulberry-purple, and ultimately to a light red-tint.

This play of colours, *under the conjoined agency of the oxidizing body and sulphuric acid*, is quite peculiar to strychnia; and, so far as is at present known, it is possessed by no other substance whatever. I will not now stop to discuss the various objections that have, at different times, been urged against the *certainty* of this test. One answer will dispose of them all, namely, that, whilst there are undoubtedly other organic principles, such as morphia, veratria, delphia, salicin, anilin, pyroxanthin, the woorara poison and others, which do yield a colour—sometimes even of a purplish cast, with sulphuric acid *alone*, there is not a single one that will strike the peculiar blue tint with the acid and the oxidizing body *conjointly*. Strychnia alone possesses this peculiarity; and I have no hesitation in regarding this so called colour-test, when carefully applied, as positive and as infallible as any other which the chemist is accustomed to employ, and upon which he ordinarily relies. I am aware that some may be disposed to question the *exclusiveness* of this assertion, and will cite the woorara (curara) poison, and its alkaloid curarina, as an exception. It is true that so distinguished an authority as Dr. Taylor, in the last edition of his work on Poisons (Am. ed., 1859, p. 680), inadvertently makes the statement that curarina resembles strychnia "in giving the peculiar play of colours when treated with sulphuric acid and one of the oxidizing bodies;" but he grounds this statement on a mistranslation from Pelikan, as has been clearly shown by Prof. Guy (vid. *Chem. News*, July 6, 1861). Besides, these two substances differ further in the fact, that whilst sulphuric acid alone gives with curarina (according to Bernard and Reynoso), a rich carmine tint, it causes no change of colour whatever with pure strychnia.

The next point to be briefly noticed is the extraordinary *delicacy* of the colour-test. It is this which gives to it its chief value. As the result of a great number of experiments made for this purpose, I have been enabled to detect with distinctness a smaller quantity than the *half-millionth* part of a grain (the precise amount was the 1-672,000th of a grain); and on some occasions, when experimenting on as minute a portion as the *one-millionth*

of a grain, I have obtained a momentary flash of the peculiar blue colour —faint, to be sure, but to the practised eye positive and real.

It is to be understood that these minute quantities of the poison can be discovered only when it is in the pure state, and free from all other organic admixture. Of course it would be in vain to attempt to detect anything like such infinitesimal portions in mixtures like the contents of a stomach. But as I have already shown, there is no difficulty of discovering as small a quantity as the *one-hundredth of a grain* (and doubtless even less), when diffused in a pint of complex organic mixture.

I will add a few words in relation to the best mode of proceeding, when we desire to test for extremely minute portions of pure strychnia.

A solution is first made of any given quantity—say one-tenth of a grain in a fluidounce of distilled water, with a few drops of acetic acid to insure solution. The number of drops contained in the portion of liquid is to be ascertained by means of a pipette, prepared by drawing out a glass tube to a fine point. The solution may then be diluted to any degree required. One drop of the liquid, representing the ascertained fractional part of a grain, is then to be placed upon a small, perfectly clean white porcelain capsule; and evaporated to dryness, either in the sun or by a very gentle heat. When cold, a drop of pure sulphuric acid is to be placed, by means of a glass rod, on the capsule alongside of the dry spot, but not in contact with it. Next, a small crystal of bichromate of potassa (or, preferably, of ferricyanide of potassium) is to be laid on the dish; after which, a clean, dry and finely-pointed glass rod should be drawn through the drop of acid so as to bring a very little of it in contact with the spot; the little crystal is next to be moved by the rod once or twice over the moistened spot, when there will immediately appear, flashing out, as it were, the characteristic blue colour—more or less transient, according to the amount of the alkaloid present, and passing through the usual tints.

It may appear to some that I have been unnecessarily minute in describing the details of what seems a very simple experiment; but I can assure such that a successful result can only be obtained, when operating upon such extremely small quantities, by the strictest attention to these details. For example, too high a temperature in the evaporation may decompose the minute quantity of strychnia, which, however, must be perfectly dry; again, the sulphuric acid should be pure and strong; for this purpose the drop should be taken by means of a glass rod out of the bottle, and *not dropped* from its rim, as it would, in the latter case, be apt to have become somewhat diluted from moisture absorbed from the atmosphere. Again, the fragment of the crystal used should be very small—one or two lines in dimensions; it should be free from all adherent powder, so as not too soon to dissolve in the acid, and thereby conceal the true colour. For this reason I decidedly prefer the ferricyanide to the bichromate, when manipulating with very minute portions of strychnia: it is much less apt to colour

the acid. Lastly, the experimenter must avoid using too much acid; hence the direction to draw only a *portion* of the drop over the strychnia spot, by means of a pointed glass rod. By carefully observing the above directions, and by a little practice, any one may soon acquire sufficient dexterity in manipulation to bring out very satisfactory results—results far exceeding the most delicate tests usually employed in inorganic chemistry, if we except the beautiful and truly wonderful analyses of Bunsen and Kirchhoff, within the present year, founded on the coloured lines of the spectrum, by means of which even the 1-200,000,000th of a grain of sodium was detected, and which led to the discovery of the new alkaline metals *cæsium* and *rubidium*. This method, however, is optical rather than chemical. Both results exhibit proofs of the extreme divisibility of matter.

Another very beautiful and scientific means of bringing out the colour-test for strychnia has been proposed by Dr. Letheby. (*Lancet*, June 28, 1856.) Ascertaining that it was the nascent oxygen which acted upon the alkaloid and developed the peculiar colours, he employed the agency of galvanism in the place of one of the oxidizing bodies. His method of proceeding is as follows: A drop of the strychnia solution is placed in a cup-shaped depression made in a piece of platinum foil. After evaporating to dryness, the spot is moistened with a drop of strong sulphuric acid. The foil is then connected with the positive pole of a single cell of Groves' or Smee's battery, and the platinum terminal of the negative pole is made to touch the acid. Instantaneously the blue colour flashes out with remarkable beauty and brilliancy. I have repeatedly performed this experiment of Dr. Letheby, and can testify to its good result in portions of *pure* strychnia as small as the 1-50,000th to the 1-100,000th of a grain; but it entirely failed in my hands to detect those *extremely* minute portions which I have before alluded to, as being discoverable by the conjoined use of the ferricyanide of potassium and sulphuric acid. Moreover, I think it open to this objection, especially in the hands of the inexperienced, namely, that all those substances which yield a colour with sulphuric acid *alone*, develop a much richer and deeper colour when subjected to the above-mentioned galvanic test; and this, in some cases, might be readily confounded with an *imperfectly developed* strychnia colour. Thus, in my own experiments with the galvanic test, delphia afforded a rich reddish or purplish-brown tint; veratria, a rich carmine, approaching to violet; salicine, a colour much resembling the last; aconitia, a rich brown; piperine, a rich orange-brown, &c. &c. All of these colours, *including the blue of strychnia*, will change to a deep brown, approaching to black, if the platinum terminal of the negative pole of the battery is kept in contact with the foil *through the acid*. The negative terminal should only touch the drop of acid, and *not* the foil. This circumstance alone is, I think, calculated to detract from the value and certainty of what otherwise is a beautiful and scientific test. Moreover, I was unable to succeed any

better by this means, in discovering small quantities of strychnia when combined with an excess of morphia, than when I employed the usual colour-test. In fact, it was much less effective; for I was unable by its means to discover the 1-20,000th of a grain of strychnia combined with 1-10,000th of a grain of morphia (double the quantity); whilst with the ordinary colour-test, as already mentioned, I could detect as small a quantity as the 1-300,000th of a grain, when associated with double that amount of morphia.

Along with the colour-test, and confirmatory of it, I must not omit allusion to the extreme *bitterness of taste* possessed by strychnia. In truth, it may confidently be asserted to be the *most* bitter substance known—as shown by the following observations, made with a view of comparison with some other well-known bitter articles:—

One grain of strychnia was dissolved in five gallons of water; a single drop of the solution, containing less than 1-500,000th of a grain, afforded a distinct bitter taste.

One grain was dissolved in ten gallons of water; a few drops held in the mouth for a moment or two produced a very perceptible bitter taste.

A third experiment still more strongly illustrates this property: A solution was made of one grain in twenty-five gallons of water; here, one drop represented less than the 1-2,500,000th of a grain. On rinsing the mouth forcibly with a portion of this solution, a faint though distinct bitter taste was perceptible. In fact, so intense and permanent is the bitter taste of strychnia, that this quality may be regarded as affording a good corroborative proof of the presence of the poison. So true is this, that I doubt if it is possible to obtain the *colour-test* in any case where the evaporated extract affords no bitter taste. Hence, its great value in medico-legal researches.

The results of experiments on other bitter substances are as follows:—

Extract of quassia came next to strychnia; its limit was a drop containing 1-358,000th of a grain—equivalent to a solution of one grain in three and a half gallons.

Carbazotic acid, aloes, and *extract of colocynth* were next in order; they all possessed about an equal degree of bitterness. One drop of a solution containing a grain to two gallons, and representing a little over the 1-200,000th of a grain, afforded a slight bitter taste.

Sulphate of quinia yielded a perceptibly bitter taste in as small a quantity as the 1-125,000th of a grain—equivalent to a solution of one grain in a little over a gallon.

Picrotoxine was slightly inferior to quinia in bitterness; the 1-100,000th of a grain may be taken as the appreciable limit.

The *salts of morphia* were vastly inferior to any of the foregoing in point of bitterness; the limit being the 1-7,000th of a grain—equivalent to a solution of a grain in half a pint of water.

I also performed a series of experiments with a view of determining the comparative value of the so-called *physiological* or *frog-test*, first noticed by Dr. Marshall Hall. There are several methods of employing this test; one is to immerse the body and hind legs of the frog in the strychnia solution; when, after a shorter or longer time, depending upon the strength of the solution and the size of the animal, tetanic spasms will be produced, owing to the poison absorbed. A second method is to inject a few drops of a solution of known strength into the subcutaneous tissue of the thorax or abdomen of the animal. A third mode is to inject a similar solution into the stomach through the œsophagus. Either one of these methods will afford satisfactory results. The first one, or that by cutaneous absorption, was, I believe, that adopted by the distinguished discoverer; and as I found it a very convenient one, I also followed it in my own experiments.

The extreme susceptibility of the frog to the influence of strychnia is truly remarkable. Long known as affording us one of the most delicate indications of the galvanic current, so as to be esteemed pre-eminently *galvanoscopic*, this little animal would appear to be no less sensitive to the action of the subtle poison now under consideration, so that it merits with equal propriety the appellation of *strychniascopic*.

In experimenting with frogs with a view to determine the presence of strychnia in minute quantities, it is advisable to make use of very small animals—those, for example, whose bodies measure from an inch to an inch and a quarter in length, and which would weigh from twenty-five to fifty grains. The simple method which I adopted, and which I found to answer extremely well, was to put a small quantity of the strychnia solution—about a fluidrachm, or even half that quantity—into a deep conical glass vessel, such as an ordinary pint graduate measure, and then place the frog at the bottom. The shape of the jar insures the required immersion of the hind legs and body of the animal in the liquid, while the head remains perfectly free for breathing. Any efforts to escape are usually prevented by the sides of the vessel; or, if necessary, it should be pushed down again into the fluid—the object being to insure continuous contact. The subjoined experiments were undertaken by me with the twofold view first, of comparing the value of the frog-test with the other strychnia tests; and secondly, of ascertaining if the presence of morphia would produce any modification of the usual effects of strychnia on the frog.

Experiment 1. A frog weighing 45 grains was put into a solution of strychnia, one drop of which contained the 1-13,500th of a grain—(equivalent to about one grain in twelve fluidounces.) Convulsions were produced in four minutes.

Expt. 2. A solution of one-half the strength of the preceding (or one grain in twenty-four fluidounces) affected a frog weighing 40 grains in five minutes.

Expt. 3. A solution of one-half the strength of Expt. 2 (or one grain in forty-eight fluidounces, or three pints) produced spasms in a frog weighing 28 grains in five or six minutes.

Expt. 4. A solution of one-half the strength of the last (or one grain in six pints) affected a frog weighing 35 grains in eight minutes. In this case the

animal was immersed only five minutes, and subsequently recovered. In the former instances they very soon died.

Expt. 5. A solution of one-half the strength of that used in Expt. 4 (or one grain in twelve pints) produced the tetanic spasms in fifteen minutes. In this case also the animal recovered.

Expt. 6. A solution of one-half the strength of the preceding (or one grain in three gallons, in which one drop represented the 1,430,000th of a grain) produced no effect on one frog after an immersion of twenty minutes; but in another, weighing 29 grains, after half an hour's immersion, decided convulsions were exhibited, and the animal suddenly died. Beyond this, I did not deem it necessary to proceed in my investigations, fully satisfied with the result; though doubtless a still more minute quantity might be shown to affect a very small animal. It may, I think, be safely affirmed that a solution containing as small a portion as the *half-millionth of a grain to a drop* may thus be recognized.

These experiments, more especially the last, illustrate very satisfactorily the extreme delicacy of the frog-test for strychnia; for certainly only a very minute portion of the poison—an almost infinitesimal quantity—could have been absorbed through the animal's skin, in the limited time mentioned. I will cite one additional experiment, under this head, which, to some, may probably appear still more conclusive:—

The 1-500th of a grain of strychnia was put into the mouth of a middling-sized frog: death took place, preceded by the usual spasms, in about twenty minutes. The abdominal viscera were removed, and treated after Staäs' process. The ultimate ethereal extract was very slightly bitter, but afforded no perceptible colour-test. On dissolving this extract in water, the solution produced the most decided tetanic convulsions on three healthy frogs, averaging 100 grains in weight each. A portion being injected beneath the skin of one, brought on twitchings in four minutes, resulting in death: a second, treated in a similar manner, was convulsed in ten minutes, but recovered: the third was simply immersed in the solution, and a small portion injected into the stomach, with the effect of producing spasms, from which, however, the animal recovered.

As a *corroborative* evidence of the presence of strychnia I should regard the frog-test as one of great importance, and one which ought never to be omitted in medico-legal researches. Taken in conjunction with the delicate colour-test already alluded to, and the bitter taste of the evaporated extract, it affords such overwhelming proof of the presence of strychnia as can admit of no possibility of cavil.

Being desirous, in the next place, of ascertaining if the presence of morphia (which I had found produced such a decided influence on the colour-test) would occasion any modification in the frog-test, I made the following experiments:—

Experiment 1. A frog weighing 29 grains was immersed in a solution containing one grain of strychnia and two grains of morphia in twenty-four fluidounces of water. Convulsive movements commenced in four minutes. Here, the proportions were one of strychnia to two of morphia.

Expt. 2. A frog weighing 40 grains, on being immersed in a solution containing half as much strychnia, but twice as much morphia, as in the former experiment (the proportion being one to eight), exhibited spasms in five minutes.

Expt. 3. A frog weighing 100 grains was immersed in a solution containing one grain of strychnia and twelve grains of morphia in forty-eight fluidounces of water. It exhibited the usual tetanic symptoms in fifteen minutes.

Expt. 4. A frog weighing 35 grains was immersed in a solution containing

one grain of strychnia and thirty-two of morphia in six pints of water. It was affected with tetanic spasms in twenty minutes; another, rather smaller, was affected in five minutes.

Expt. 5. A cat was poisoned by taking one-twentieth of a grain of strychnia and one-tenth of a grain of morphia (double the quantity): convulsions and death took place in about thirty minutes. The stomach was, on the next day, analyzed by Staäs' process. The ethereal solution, although concentrated, totally failed to yield the colour-test (as already mentioned), but the extract possessed a slightly bitter taste, and its watery solution produced the most decided convulsions (generally resulting in death), in *eight* distinct frogs, averaging 100 grains each. Some were merely immersed in the liquid; to others, it was administered either by injecting into the stomach, or under the skin.

It is abundantly evident from the above experiments, that very little, if any, effect is produced by morphia in modifying the influence of strychnia upon frogs; consequently, the presence of morphia, although most seriously interfering with one of our means of detecting this poison, viz., the *colour-test*, most fortunately produces no impression on the almost equally delicate *frog-test*.

Since making these very delicate experiments with frogs, I have regretted that I was unable to apply this test in the poison case mentioned at the commencement of this paper; but I was unfortunately unable to procure any frogs of the proper kind in the winter season, when the examination was made.

Before concluding this already lengthy paper, I will add a word or two in reference to the *microscopy* of strychnia—a subject which, I believe, has not heretofore been particularly noticed. Having ascertained the extreme delicacy of the other modes of testing for this substance, it occurred to me to examine minute portions by means of the microscope; and the results obtained were exceedingly beautiful and satisfactory. I have succeeded best by evaporating a drop of a solution in pure water, of known strength, on a glass slide, and subjecting it to the field of a good instrument. The solution must, of course, be perfectly pure, and free from all organic matters. Very satisfactory results may be obtained by using quantities as small as the 1-50,000th to the 1-500,000th of a grain; although even the 1-1,000,000th may easily be recognized. The appearance presented to the eye is that of numerous crystals, some acicular and others stellate and scalloped, intermingled with dentated crosslets; the whole bearing a striking resemblance to the appearance presented by the arborescent crystals of the triple phosphate seen in a drop of evaporated urine.

It is not pretended that we should rely on the microscopic appearance as a test for strychnia *per se;* it can only be employed as a corroborative proof; but its delicacy and its beauty should always justify its employment, so far as practicable; although it must not be forgotten that it is available only when the alkaloid is in a state of purity.

ART. VII.—*Successful Operation for Inguinal Hernia with the Testicle and Sac both above Poupart's Ligament.* By F. TAYLOR BRADFORD, M. D., of Augusta, Ky.

THE unusual character of the present case, together with the mortality attending the operation for hernia, renders the discussion of the subject, as well as each individual case, a matter of peculiar interest.

Whilst *Amussat*, and his long list of followers, insist upon "subjecting the hernial tumour to continued pressure and kneading for *days* together," *Desault*, and his converts, teach us to "think favourably of a hernia which *has not* been *handled* before the operation."

"Has the practice of opening the sac had any influence on the great number of unfavourable results?"

Petit, Le Dran, Munro, Sir Charles Bell, Bransby Cooper, Aston Key, Luke, and *Gay*, say yes; *Dupuytren, Richter, Hey, Heister, Sir A. Cooper, Lawrence, South*, and *Hancock*, say no!

We are continually drifting to extremes, and it is only by the "*second sober thought*" that we may move steadily by the legitimate landmarks of safety.

The fact that a solitary instance here and there has yielded to the oft-repeated efforts by purgation, the taxis, and a long delay for the hopeful chances of nature, is not of itself *prima facie* evidence that the rule is founded in conservative surgery.

Those who have read the very excellent work of Mr. Hancock cannot come to any other reasonable conclusion than that the great fatality of hernia is owing, not so much to the operation, or the manner in which it is performed by good surgeons, as to the delay incident upon continued and repeated efforts by the taxis, purgation, and rough and repeated manipulation of the part. If my limited experience is worth anything in matters of this sort, it is this: Place the patient under the influence of chloroform, embracing as it does the advantages of venesection, tobacco, the warm bath, tartar emetic, opium, &c. &c.; then make an effort by the taxis, and if you do not succeed, the indications are pretty clear that you have neglected the proper position of the patient, the best means of reducing, or that an operation should be advised or performed. My only object in making these remarks is to guard the practitioner against a *common error* that days must be spent in efforts by purgation, taxis, &c. &c., until the reasonable hopes for a timely operation are gone. They are not made, however, with a view to the present case. It was in the hands of intelligent men, and from this fact, perhaps, more than to the manner of the operation, does its success depend.

The subject of the present interesting case was a negro man, aged 45, of

good constitution, whose muscular development was most extraordinary; his remarkable feats of strength and muscular prowess rivalling perhaps anything of the kind in the State. He had a wife and several children. A few days previously he was shaving a *mall* with a draw knife, having one end against his belly; suddenly he complained of *colic pain*. I found him with swollen abdomen; sense of tightness across the lower part of the bowels; frequent desire to go to stool; inability to evacuate; occasional vomiting; hiccough; countenance anxious; pulse small, hard, and wiry. Immediately above *Poupart's ligament* was a large swelling, tender to the touch, slightly elastic, with a glossy cast as though it was oiled. The swelling was *peculiar*, and lay across the bowels, not perpendicular, measuring perhaps five inches in length. Upon examination of the *scrotum* only one *testicle* was found. When interrogated as to this state of affairs, he stated "*he never had had but one.*" He then stated that twelve years before he had suffered in the same way, and after a day or two a physician had got his bowels acted on, and said he had reduced the bowel. But from that time up to the period of this attack "*he had always had a lump there;* it never had gone away."

Drs. Duke and Sharp, of Maysville, and Taylor and Cartwell, of Washington, were in attendance; they had used the most efficient and prudent means to reduce the supposed strangulated bowel without effect. The known ability of these gentlemen, and the time which had elapsed, deterred me from further irritation of the part; but believing that the *testicle* lay above Poupart's ligament, and that inflammation of that organ at that point might give rise to a similar train of symptoms, and somewhat influenced by his former attack, I suggested the propriety of making a further attempt to move the bowels and delaying the operation a short time. It was cheerfully agreed to. The present condition and history of the case was the history of strangulation, *critically considered*, yet the former history of his attack, where the bowels had acted, influenced us to this conservative hope. The bowels, however, not being acted upon at the expiration of the time agreed upon, the patient was placed under the influence of chloroform, and the operation commenced. Taking hold of the integuments immediately over the tumour with the left forefinger and thumb, lifting them up slightly and guiding them up and down so as to be sure they were freed from the sac, I thrust the knife through the integuments with the back next the tumour, and cut directly upwards and outwards. This brought me near the sac, and of the remaining membranes (*intercolumnar fascia, &c.*) a little bit of each was pinched up by the forceps and cut into. A grooved director was then placed in the small *niche*, and each membrane carefully divided until the sac was reached. Its bluish transparent coat was divided like the former coats. The left forefinger was then passed into the neck of the sac; the stricture being found, the hernia knife (Le Gros Clark's) was passed up flat on the finger through

the stricture, the edge then turned up, and the division made by a slightly *undulating* motion *directly* upwards—knife parallel to the *linea alba*.

The stricture was remarkably firm and tendinous, and was distinctly heard when divided by the knife. The sac contained a small quantity of limpid serum. A small portion of the bowel was of a deep purple hue, interspersed with spots, and a *knuckle* of an ash hue which gave rise to some uneasiness. The bowel being returned into the abdomen brought to view the *fugitive testicle* lying immediately under the sac. It was much smaller in size than the other testicle, but perfectly healthy in colour, and firmly adhered to its strange locality. I did not attempt to remove it. The wound was dressed in the usual way, and the patient recovered without much trouble.

Dr. Taylor and Dr. Cartwell are entitled to much credit for their faithful attention after the operation.

ART. VIII.—*Case of Internal Necrosis after severe Injury of the Thigh, with extensive Chronic Sub-periosteal Abscess; Operation; Cure.* By H. O. HITCHCOCK, M. D., of Kalamazoo, Michigan.

MR. P. V. W., aged 37 years, an architect and builder, presented himself at my office Nov. 15th, 1859, for examination and advice relative to his right thigh. He gave the following history: Five or six years ago, while raising the roof of a church in Hudson, N. Y., he received a severe raking injury in the right thigh, by the falling of heavy timbers upon its outer aspect. At the same time there was fracture and dislocation of the left ankle. A surgeon, soon in attendance, adjusted the fracture and dislocation, and "smoothed out" the muscles of the right thigh, which seemed torn from their origin and gathered in a heap under the skin. The injury was then considered only muscular.

A few weeks after the accident the patient began to suffer very severe pain in the right thigh, as if in the bone. This, he says, was considered and treated as a rheumatic pain for three weeks, when, choosing to die rather than be in such agony longer, the patient insisted that the attending physician should lance it. Two small openings were made on the posterior border of the biceps muscle, about midway of the length of the femur. These gave exit to a large quantity of sanious, watery pus, and afforded instant relief to the patient. These openings have never been closed, but continued, till the day I saw him, to discharge pus of the same description.

Six months after the accident, the patient was able to resume his employment, which he has since continued, though with much pain and great

discomfort. While in the upright position, at first, but little oozing took place, but the lower half of the thigh would soon become swollen and painful "as if," in his own expressive words, a "dog were gnawing it." And when the abscess was nearly filled, every contraction of the muscles of the thigh caused the matter to flow from the sinuses. In hot days in the summer, this discharge was often immense, and at night he would close the labours of the day by evacuating the pus from the lower part of his thigh with the foot elevated.

Such had been his life for the past five years. He had lost much flesh and much of his spirit and energy. He was warned just before he came to my office, by a painful swelling of the inner condyle, that the disease was about to invade the knee-joint. Hence his application for advice.

The patient is a large well-developed man, with no taint of scrofula or syphilis, of a pale sallow complexion, with an anxious expression of countenance indicative of long-continued suffering. The sinuses were empty, and the probe followed them easily to denuded bone. But so straight and narrow were the sinuses, it was impossible to determine whether the instrument impinged upon a sequestrum or not. An exploratory operation was advised, to which he readily submitted, it being agreed, however, that any operative procedure necessary for cure should at once be completed.

On making an incision of ten inches in length between the biceps and vastus externus muscles, commencing one and a half inches above the knee-joint, the bone was found bare of its periosteum throughout almost the entire length of the wound, and more than one-half the circumference of the bone.

What might have been the periosteum, was thickened, jelly-form, and seemed covered with a smooth, shining membrane. The surface of the bone was granular and somewhat crumbly. This surface was chiselled off to the depth of one-eighth to one-fourth of an inch, until healthy tissue was reached. The gelatinous covering above was also removed as far as possible.

In the upper half of the middle third the femur seemed decidedly and somewhat abruptly enlarged, even to nearly twice its natural size. On searching with care, there was found a hole about one-fourth of an inch in diameter, leading directly into the centre of the bone. This also was filled with the same jelly-form substance as was noticed to cover the bone. The bone was at this point trephined, and the medullary cavity scraped as the surface had been. And I was gratified in withdrawing from the centre of the bone a sequestrum one inch long, one-third of an inch wide, by one-sixth of an inch thick. The clearing process was carried on till nothing but healthy tissue could be brought from the medullary cavity.

It should be remarked that the thickness of the bone at the point of trephining was fully three-fourths of an inch, and very compact and hard.

The wound was now well cleansed, and closed with a pledget of lint throughout its whole extent, to insure granulation from the bottom.

The patient was attacked in his weakness, by that most lupine of all diseases, ague. The progress of healing was very slow, and about six weeks after the operation the internal condyle became swollen and painful. This was laid freely open, and we found, on the inside of the thigh, a very extensive abscess with the same jelly-form lining, communicating at its upper part, over or anterior to the bone, and at its lower part, under or posterior to the bone with the exterior abscess. Astringent injections of zinci sulphatis gr. v, ad ℥, and hydrarg. bichlorid. gr. i, ad ℥, were used with the most gratifying effect, so that on the first of April the disease seemed wholly eradicated, and the wound almost entirely closed; and our patient entered upon a very large contract of heavy buildings, in better health and spirits, and more fleshy than he had been for five years. To-day, two months later, the wound is completely healed, and the leg seems perfectly sound.

This case presented to me some points of interest:—

1*st*. Was there not a fracture of the femur at the time of the accident, and hence the internal necrosis?

2*d*. Would not free incisions through the periosteum, at the time of the supposed rheumatic pain, have saved the patient from all the trouble of the sub-periosteal abscess?

3*d*. The value of a solution of bichloride of mercury as an injection in old sinuses and chronic abscesses.

4*th*. The fact that no periosteum will be formed to a large extent.

KALAMAZOO, MICHIGAN, June 1st, 1860.

TRANSACTIONS OF SOCIETIES.

ART. IX.—*Summary of the Transactions of the College of Physicians of Philadelphia.*

1861. *Feb.* 6. *Microscopic Structure of Diphtheritic Membrane.*—Dr. STROUD exhibited a diphtheritic membrane of tubular form taken after death from the air-passages of a child of ten years of age, who had died, after five days' illness, of diphtheria.

This specimen presented the usual appearance to the naked eye, but it had been carefully examined under the microscope by Dr. James Darrach, to whom Dr. Stroud referred for a particular description.

Dr. J. DARRACH remarked that the membrane presented by Dr. Stroud exhibited, under the microscope, corpuscles and small round cells, with single nuclei like young epithelial, also granular matter. There was no appearance of fibrillation. In further illustration, Dr. D. then presented a specimen, removed from a chicken, exhibiting a deposit allied to that of diphtheria. The deposit was upon the mucous membrane of the sides of the mouth, underneath the tongue, upon the palate, and within the glottis, which had been entirely closed by it. The chicken, *no doubt*, died from suffocation. This chicken lived about eight days from the time when it was first noticed as sick, during which time it refused to eat, but drank a great deal of water. Since noticing the above, the Doctor has examined another chicken which died of the same affection. In this instance the whole nares were involved. The deposit, however, was found directly upon the rim of the glottis, and not within it. In neither of the above did the deposit extend below the glottis. What, perhaps, is worth noticing is, that all the children (some four or five) of the family which owned these chickens, had just recovered from diphtheria. This affection was also prevalent in the neighbourhood.

The Doctor mentioned that he did not wish it to be understood that he thought the disease of which this chicken died differed from one already recognized as occurring in poultry, from which the term croup was obtained. He stated that he lacked information on this point. At the same time he desired to direct the attention of the college to the close resemblance of this disease to the diphtheria in the human subject both as to the elements of the membrane and the locality affected by the exudation.

The Doctor made the following remarks upon the structure of the diphtheritic membrane. He was disposed to doubt that fibrinous exudation was a constant effect in diphtheria, or even in ordinary sthenic croup. This opinion was founded on observations made by himself and his brother, Wm. Darrach, Jr., during the epidemic of this disease in Philadelphia, in the winter of 1860.

He had examined a number of the patches of membrane, which were carefully removed during life, also others taken from cases after death.

The elements found in all these examinations were cells, young epithelial

nuclear bodies, not affected by acetic acid, and, most likely, aborted epithelial cells, with pus-corpuscles and granular matter; the granules occasionally assuming a disposition to form themselves in parallel lines.

In no one case could be perceived a distinct fibrillation, as is easily recognized in the transudations upon the pericardium and other serous membranes.

If the fibrillation be not present, we have not the morphological element of coagulated fibrin; and, therefore, unless there be some chemical test by which it may be designated, we cannot predicate its existence under the microscope.

It is true that a granular form of fibrin has been described, but this has not been established.

Again, has not the existence of fibrin in some of the membranous patches of diphtheria, and in other affections of the mucous membrane, been rather inferred than demonstrated?

We have been taught and many still maintain that pus and other cell-elements are the direct product from an unorganized blastema termed fibrin; more especially has this been considered the origin of pus, at least with those who have given up the idea of its being a secretion.

The existence of these elements has led observers to predicate a substance (fibrin) from which these elements have originated.

But granting this view, to which we have become so wedded, to be no longer tenable—and if Virchow substantiates his statements, it no longer is—then we need not presuppose the existence of fibrin because we see nuclear growths or cells, now termed pus or exudation-corpuscles, which, according to some, are the elements by which the exuded fibrin is destroyed.

Is there not another view which may satisfactorily explain the nature of at least some of the formations which are found upon mucous membrane?

Are they not frequently simple products of perverted cell-growth, which, owing to a poison to be eliminated, or from some other unknown cause, gives rise to rapid multiplication of aborted epithelial cells, these being formed into membrane of a low vitality which soon disintegrates into granules.

Nor is it by any means an unsupposable case that cells which are arranged in rows, should leave some such appearance in the granules, the product of their disintegration.

These views have been confirmed by the examination of the deposit in the chicken already reported.

Another reason which might be adduced in their support is that mucous membranes are not disposed to transude fibrin. Mucous inflammation tends to the catarrhal form, and results in the formation of a tenacious albuminoid product, which is considered by modern pathologists, not as a *transudation*, but as a *product* of cell-formation. This excessive cell-action causes a rapid cell-growth and terminates in a degradation of the individual cell from exhaustion.

Note from Dr. G. B. WOOD *in relation to the Calabar Poison Vine, and on the Mode of Gathering the Resin of the Indian Hemp.*—Dr. CARSON stated that he had received a letter from Prof. G. B. WOOD, the President of the College, transmitting some of the seeds or fruit of the Assafœtida plant, which had been presented in Edinburgh to Dr. Wood by Drs. Christison and Balfour, the latter of whom is Professor of Botany in the University of Edinburgh, and Superintendent of the Royal Botanical Garden of that city.

Dr. Wood mentions having seen in this garden, during a visit in company with Drs. Christison and Balfour, among other novelties, an undoubtedly *genuine gamboge plant*, which had been brought or sent to Dr. Christison from Siam, its native country. As no flower was received, however, and as many years must elapse before the young plant they have will produce one, its precise botanical character has not been determined.

By permission of Dr. Wood, the following extracts from his letter were read by Dr. Carson to the College:—

"Some time since Dr. Christison received some of the seeds of the famous Calabar poison vine, used by the natives of that part of Africa in their trials for witchcraft; and, having taken about half of one of these seeds, he was so violently affected that his friends were alarmed for his safety. Some of these seeds were planted and produced a climber, which flowered in due time and enabled Dr. Balfour to determine its character. He named it *Physostigma venenosum*."

"Another point of some interest is relative to the mode of gathering the resin of the Indian Hemp by the natives. Dr. Christison says that he has learned directly from good authority in Hindostan, that this method consists in rubbing the leaves and flowering tops between the hands, and then, when these are sufficiently loaded with the adhesive exudation, to scrape it off. The prevalent notion that the natives gather the resin by walking through the fields of hemp, and afterwards scraping off the matter which adheres to their clothing, is, to say the least, apocryphal; nay, he considers it absurd, as the parts of the plant which exude the resin are above the heads of the natives, and the plants grow so closely together and are so strong that a native could hardly make his way through a field of them. He says, moreover, that, according to information he has received, the hemp plant does not produce the resin satisfactorily in the low hot plains of Hindostan, but in the regions near or among the hills. I have little doubt, myself, that it might be advantageously collected from the hemp of our own country; as specimens grown in my garden have had strongly the characteristic odour, and the tops have been very adhesive to the hand; which was not the case with a specimen I saw growing in the Botanical garden here."

Fatal Parturition from Inertia of the Uterus; Fœtus not delivered.—Dr. CORSE read the following note of this case, and exhibited the fœtus and its membranes with the uterus: Mrs. A. P. was taken in labour at a period which she supposed to be the close of the eighth month of pregnancy.

The uneasy feelings usually preceding labour began gradually, and continued for several days without any increase of the pain or uterine contractions. She at length began to feel spells of sinking and prostration, which were at first slight and of short duration, but gradually became more marked and of longer continuance; their recurrence also became more frequent. No hemorrhage whatever took place, not even the usual amount which sometimes constitutes the *show* of obstetricians. I was sent for in consultation; but a few minutes before I arrived, one of her sinking spells came on, in which she expired.

Her colour at that time was exceedingly pallid. No patches or spots of purple or deep red appeared, as in cases of purpura and some other diseases, incident to a low state of the system. The body was plump, and the skin free from the shrunken and wrinkled state which it presents sometimes in cases of death from extreme prostration or exhaustion. She had had no

diarrhœa or other exhausting discharge. A moderate quantity of food had been taken daily, and she had had a tolerable amount of sleep in short naps, but no long or refreshing sleep at any one time for several days.

The emotional system being depressed, for some weeks before her death she had been labouring under dejection of spirits. No good reason could be given to account for this, and we are left to suppose it to have been, for the most part, due to the remembrance of a former labour about two years before, from which she narrowly escaped with her life. In that labour she had had a placenta prævia, and was reduced by the hemorrhage to the last extremity, and lay many hours after delivery before reaction could be brought about. I was with her on that occasion, in consultation, and delivered the infant by version from a vertex presentation to a footling. Soon afterward some of her lady acquaintances very indiscreetly predicted that, if she should ever become pregnant again, she would surely perish in the delivery. It is not positively known that this preyed upon her mind, but she was meditative in her character and of taciturn habits. No other cause for depression of spirits is known to have existed.

Permission for a *post-mortem* examination was obtained, in hopes of finding some other cause of death, but our expectations were disappointed. I opened the body by incision near the median line of the abdomen, as is sometimes done in the Cæsarean section, and on cutting into the uterus found an anterior implantation of the placenta. The fœtus was in the second position vertex presentation and of large size, yet no obstacle to the delivery presented itself. No appearance of the fœtus induced the supposition that it had been long dead. The viscera of the thorax and abdomen were in turn examined, and all of them found in a healthy state, with the exception of congestion or engorgement which prevailed in all the internal organs except the uterus; this did not appear to have had more than the usual supply of blood. We are led to suppose that the depressing emotions have acted in this case on the general nervous system in such manner as to bring about inertia of the uterus. Such depression, however, is not necessarily the cause; since inertia of the uterus may occur in patients whose bodily strength is good. In other instances, also, where great prostration of the general system exists, the tone of the uterus is maintained and it acts vigorously. Admitting the inertia of the organ, from whatever cause, the presence of the fœtus is naturally exhausting; and *any* depressing cause, acting coincidently, would tell with twofold effect upon the vital powers. Should, under these circumstances, a syncope occur, the presence of the fœtus would be greatly against a successful reactionary effort.

The os uteri remained undilated, and was only about three-quarters of an inch in diameter; delivery, therefore, was out of the question.

Besides its pathological value, the *post-mortem* specimen here presented has a physiological worth. In it we may see the mode and extent of the attachment of the placenta to the uterus; the great size of the uterine arteries during pregnancy; the calibre and situation of the uterine veins, now called uterine sinuses; their greater capacity opposite the placenta, etc. etc.

Inspection of the ovaries would lead us to regard the left one as that which furnished the ovule for this pregnancy, there being in it a well-marked corpus luteum.

March 3. *Fatty Degeneration of the Placenta, resulting in Abortion. Fœtus retained Two Months after its Death.*—Dr. CORSE read the following sketch of the case, and exhibited the specimen :—

Mrs. H., pregnant for the third time, sent for me, on account of excessive menstruation, as she supposed. She stated that she had taken cold some time since, and, in consequence of it, had passed her last three menstrual periods without any discharge. About two weeks ago they had come on again, and still continued.

With the discharge she had only very little pain, but great weakness. Examination of the mamma showed the areola and papillæ indicating pregnancy; a per vaginam examination revealed an enlarged uterus; and some other signs, with these, induced me to express the opinion that she was pregnant about three months. She was reluctant to suppose herself in this condition, and said that her feelings were different from what they had ever been before. Under appropriate treatment the hemorrhage abated, but did not cease entirely. Several interviews followed, and examination showed the areolæ and papillæ increasing and the uterus enlarging, up to the end of the fourth month; at this time I noticed that the mammæ suddenly became smaller, and the areolæ faded considerably away; the uterus diminished in size, and the feel of it entirely changed. The hemorrhagic discharge had changed to a bloody serum which oozed away in no great quantity, but still enough to annoy the patient. These symptoms went on a little more than two months longer, and then she was taken suddenly in the night with profuse hemorrhage from the uterus. Pains soon came on, and in the course of a few hours she was delivered of the fœtus and membranes which I here exhibit. The placenta presents, all over its uterine aspect, a surface free from the tufted and villous character of the healthy organ; it is dense, of a suety hardness, and has very little elasticity. The thickness of the hardened portion is from two to three lines—the part of the organ between this and the fœtal surface being composed of the usual fibrous tissue, with interlacing bloodvessels.

Microscopic examination showed this structure to be fibroid, with much intervening granular, fatty matter; with difficulty could anything like villi be made out. Numerous granular corpuscles were visible, and some few nucleated organic cells were to be seen.

The attention of the profession was first called to this form of disease about seven or eight years ago. Since then the subject has been more closely investigated, and many pathologists are of opinion that the fatty state is secondary, the primary one having been some form of disease (probably inflammatory), accompanied with a copious exudation of fibrin. It is well known that these products of such low organization are very prone to run into a state of degeneration, with much oily matter, in a granular state, throughout.

Recent observations have shown masses of fatty degeneration to be very common in the placenta at the close of utero-gestation; and this has led to the supposition that this state is one of the phases of retrograde change normal to tissues whose period of existence has expired. This idea, however, cannot apply to cases of fatty degeneration of the placenta occurring in the earlier months of pregnancy.

Pus is said to be fatty degeneration of the exudation-corpuscles; and this might, without impropriety, be regarded as a state of degradation just short of pus; one of the stages in the process of cure where the vitality was not high enough to remove the fibrinous deposit by absorption, and not low enough to run into pus.

It would seem to me probable that this state was brought about by implantation of the placenta over a part of the uterine mucous membrane,

which was in a state of inflammation; the endo-metritis would incline to set up the same action on the adjacent part of the placenta, and thus cause the deposit of fibrin. An effort towards cure on the part of nature converts it into fatty degeneration; but before the purpose is accomplished, the death of the fœtus takes place; and, after a greater or less period of time, this is expelled.

Case of Cold-stroke.—DR. HENRY HARTSHORNE gave a brief account of a case of fatal illness following sudden exposure to cold for a short time.

J. S. H., aged 14, previously in ordinary health. On the night of the 7th of February (on which day the thermometer had gone down nearly 50° Fahr. in twelve hours, with the rise of a piercing wind), he rose from his bed in the back chamber of a large house, and went, barefoot and in his night-shirt, to the window of the adjoining front room, which he opened and stood by for a few minutes, looking out, on account of an alarm of fire. Being thus thoroughly and suddenly chilled, he had some symptoms of indisposition the following morning, but became more ill as the day advanced. Dr. H. H. saw him about 5 P. M.; at which time he was suffering with headache, drowsiness, and vomiting; the skin moderately hot; the pulse hard and accelerated. A saline laxative had produced two evacuations of the bowels. Fourteen leeches were directed to be applied to the back of his neck, cold cloths were placed upon the forehead, and the solution of citrate of potassa was prescribed.

The next morning he was still suffering with severe headache; although his skin was of a lower temperature, and the hardness of the pulse had subsided; its rate being 95 per minute. In the afternoon he vomited twice; his bowels were also freely moved. The cold applications and neutral mixture were continued, and mineral water was allowed as a drink; a mustard plaster, also, being applied to the back of his neck.

About 6½ P. M., he became extremely restless and incoherently delirious, setting his teeth firmly against food or medicine, but opening them to receive and chew lumps of ice. He recognized no one except his mother. There was no clonic convulsion, but a disposition to clasp the fingers tightly against the palm, and to throw out the hands and feet with impatient suddenness. The skin at this time was *moist;* the pulse variable in force, but constantly accelerated.

Under the advice, in consultation, of Dr. Charles Evans, a blister was applied to the nucha at 10 P. M. It was found impossible to compel him to swallow any medicine. At about 4 o'clock of the succeeding morning he died. A *post-mortem* examination could not be obtained.

After careful inquiry, it was impossible to discover any probable cause for this sudden and rapid illness except his exposure, upon leaving a warm bed, to the piercing wind of a cold winter's night.

Such results of cold are undoubtedly rare; but similar cases have been recorded. A London periodical, quoted in the March No. of the *N. Am. Med.-Chirurg. Review,* mentions the case of a gentleman who, on a very cold day during the past season, was observed by a friend in the street to look ill; he was sent home in a cab, became delirious, and died in twenty-four hours. The number of sudden deaths in London, during the same cold spell, is believed to have been greater than at any previous period; while the aggregate general mortality was also excessive.

Larrey, in his memoirs, speaks emphatically of the importance of sudden and great *changes* of temperature in producing deleterious effects. He

asserts that gangrene from congelation never occurs unless the body or limbs have been, after freezing, rapidly brought to a high temperature. The reverse of this was illustrated in the case above narrated: or, perhaps, we might more correctly say, that it was the converse or opposite of sun-stroke or heat-stroke. Probably the rapid and extreme change of temperature disorganized the minute elements of important structures, especially of the brain; in a mode analogous to that in which congelation, especially when suddenly succeeded by the thawing process, destroys the vitality of plants.

A New Elastic Pessary.—Dr. H. HARTSHORNE also exhibited to the college several *ring pessaries*, of different sizes, made of vulcanized caoutchouc. The material and form were almost precisely those of the ordinary gum-ring given to infants. Dr. H. H. had first made use of the gum-ring as a pessary, and found the only objection to it to be its deficient size. The pessaries exhibited had, therefore, been made to order, at his request, through the agency of Mr. Hubbell (1410 Chestnut St.), by the Goodyear Rubber Company. In the cases in which he had so far used them, he had found them to answer perfectly well. After remaining *in situ* for many weeks, they had been found to show less evidence of change than many of the gutta-percha rings would have done.

The advantage of this kind of supporter is, in a word, its elasticity; which imitates and substitutes (or affords a supplement to) the natural supporting powers of the utero-vaginal apparatus, more benignantly than any inflexible or unyielding material can do. At the same time it is not unreliable, as is the case with the India-rubber air-bag or colpeurynter, and is free from the annoyance of a protruding tube.

Although medical men differ very much in their estimation of the value of pessaries in the treatment of uterine displacements, it would seem that the most obvious objections to their use must be removed or weakened, if we have at hand instruments which closely imitate the natural ligamentous tissues, in affording an elastic support. The same yielding or flexible character, moreover, makes it easier for the patient to introduce and remove the pessary for herself; an advantage, in many cases, of no slight importance.

June 5. Perineal Fistula, with Abscess of the Testicle.—Dr. CORSE read the following note of a fatal case, and exhibited the *post-mortem* specimen:—

Mr. H., æt. 44, sent for me, in consequence of daily chills, fever, and great prostration. I found he had a fistula in perineo, from which the urine constantly dribbled; none passed by the urethra. From him I learned that he had always had a weakness in the bladder from boyhood, and that his mother died early from phthisis. About the age of six years he received a hurt on the left hip-joint; this was followed by inflammation, which became chronic, ran into hip-joint disease, and finally resulted in anchylosis of the joint. He had suffered frequently from disorder of the urinary organs, with great pain and difficulty in passing urine. In the summer of 1859 the fistula opened, and he passed water both by the urethra and by the fistula. In July, 1860, he underwent an operation for the cure of the fistula. This hope was not realized, but, on the contrary, he found himself worse rather than better. In this unhappy state he removed to Philadelphia in the autumn of 1860, and placed himself under my care. Of the operator or of

the operation performed I have no knowledge, and his condition was not such as to justify me in undertaking any operative treatment. I therefore applied myself to improving his general health by means of tonics, nutritious diet, and stimuli. He rallied and regained, in some degree, his strength; soon, however, an attack of epididymitis set in, and ran into suppuration in spite of all efforts to prevent it. Great prostration followed this, and no course of tonics and nutritious diet seemed to bring him up; the abscess in the testis continued to discharge pus by a fistulous opening which had formed through the dartos. Not long afterward several carbunculous boils formed in different parts; he began to run down in spite of the very best diet, tonics, and stimuli, and at last expired.

A *post-mortem* was allowed, by which I obtained the specimen I here lay before the College. The first incision was made by placing the point of the scalpel about the middle of Poupart's ligament on the right side, and carrying it in a semilunar direction through the hypogastrium to the middle of Poupart's ligament on the opposite side; another incision was then made in the median line of the abdomen from the ensiform cartilage, to join the one already made. These incisions were deepened and extended through the peritoneum, so as to open the cavity of the abdomen; then the cuts on each side were carried downwards through the soft parts into the anus, thus inclosing the whole of the region involved exteriorly. The soft parts within the pelvis were then separated from the sides, and the rami of the pubis divided by means of a saw, the section passing through the obturator foramen near its middle. The piece now being loose, the remaining soft parts were divided down to the bottom of the pelvis, and the entire mass removed, containing the whole of the genital organs.

On examination it will be found that the bladder is reduced in capacity to about one inch in diameter; the walls are thickened and indurated, and have lost their elasticity. The whole mucous surface is covered with granulations; no part of the normal mucous covering being left. The fistulous canal is large, and occupies the place of the membranous portion of the urethra, which is almost gone by ulceration. The point at which the unnatural opening in the urethra has taken place seems to have been behind the triangular ligament; the liquid has followed that structure to its lower edge, and, passing under it, has made its way out about the centre of the perineum.

The abscess which has formed in the left testicle does not connect with the primary fistula, but it seems to have been caused by extension of the inflammation from the urethra through the vas deferens; for the one on that side is much the larger and yielded pus under pressure, while the smaller one on the opposite side yielded, on compression, a white milky fluid.

The ureters here are both very large, and yielded pus on pressure. Their mode of entrance into the bladder has lost its characteristic valvular arrangement; they enter directly through the walls, and present on the inside a large and ragged orifice.

The prostate gland seems to have been the seat of some inflammatory action, as several cavities are here to be seen, which contained pus.

The seminal vesicle of the left side is much thickened in its walls; the cavity remains open. The vesicle of the right side is one indurated mass, the cavity entirely obliterated.

The urethra has been cut open throughout its whole length, and the point of constriction shows that the unnatural opening does not necessarily take place close to the constricted part, the distance between them being, in this instance, more than three and a half inches.

Art. X.—*Summary of the Proceedings of the Pathological Society of Philadelphia.*

1860. *Feb.* 13. *Pseudencephalous Fœtus.*—Dr. H. Lenox Hodge exhibited a monster of this variety, the offspring of a patient under the charge of Dr. S. R. Morris. The body and limbs were remarkably well developed. The mother had previously given birth to a perfectly normal child; it could not be ascertained that any untoward circumstance had occurred to her during her pregnancy.

Respiration and circulation were established in this child, and continued for twenty-four hours; reflex action was readily excited. The tongue was constantly protruded, as if in attempts at sucking. About twelve hours after birth, the child had a convulsion, apparently brought on by its attempting to cry. A second convulsion occurred about twelve hours later, and was followed almost immediately by death.

Upon careful dissection, the state of the head was found to be as follows:—

The occipital bone is about half its natural size, its upper angle being doubled down so as to form a rounded edge just above the attachment of the tentorium. The occipital foramen is complete at the posterior border, and just above it there is a sharp projecting spine. The bone rises almost vertically behind this foramen, while in front the basilar process ascends nearly as abruptly, and is devoid of the usual groove for the medulla oblongata.

The temporal bones present their petrous portions running forwards as usual, but inclining very much upwards. Their squamous portions are but little developed.

The parietal bones are present, but doubled down like the occipital, and very slightly developed.

The sphenoid is almost entire, but the greater wings are wanting at their outer portions, where they should form part of the cranial wall. The optic foramina are very small, and the upper surface of the body of the bone continues the nearly vertical direction of the basilar process of the occipital.

All the other cranial bones are wanting.

The lower portion of the malar is present on either side.

The upper maxillary bones are partially developed; the left is larger and projects more forward than the right. In front, as they approach each other, they are rounded off below, leaving a gap between them. In a fibrous capsule at the anterior and upper part of each, there are two incisor teeth. The nasal tuberosities are wholly absent.

The lower maxillary bone is well developed.

From the above description it will be perceived that there was in this case a virtual absence of the cranium. A tentorium existed, closely connected to the bone below by fibrous tissue; above this there was no solid material to indicate the brain. About opposite the lower border of the axis, the spinal marrow was present, surrounded by pia mater; it was very soft, perhaps owing to the length of time the specimen had been kept. It is certainly wonderful that this child should have breathed for twenty-four hours.

Dr. DARRACH presented a tumour removed by Dr. Pancoast from the velum pendulum palati of a young woman æt. 20, at the clinic of the Jefferson College.

The growth was of the size of a hen's egg, and extended below the dorsum of the tongue on the side of the pharynx. It was of some years' growth, and constantly impeded deglutition, causing also at times a good deal of difficulty in respiration. The patient was endowed with a good constitution, and the tumour, which fluctuated slightly to the touch, was thought to be cystic in its character; its removal was therefore decided on. It was drawn forward by means of a large tenaculum, and the mucous membrane over it divided perpendicularly with the knife. The cyst thus exposed was white-coloured, thick, and resisting. After puncturing it freely so as to discharge a portion of the fluid it contained, the sac itself was easily detached from the surrounding parts with the end of the finger, and removed in a mass. No bleeding of any consequence followed, and the patient recovered very speedily.

Dr. Darrach examined the growth, with the following results: It consisted of a sac, composed of fibrous tissue, which contained a substance varying in consistence and colour from that of semi-hardened glue to that of a diffluent jelly. Portions also appeared to be undergoing degeneration. By everting the sac the contents were extruded, as in peeling a banana. Its form was an elongated oval, its size about that of a hen's egg. Upon more careful examination of the firmer parts of the tumour there could be seen delicate opaque white nerve-like bodies, separable from the rest of the mass by a little care. The softer portions adhered to the finger and could be drawn out like mucus or pus treated with an alkali. Acetic acid rendered them opaque and firmer. Boiling in water had no effect.

Microscopical examination.—From the varied and peculiar characters of the elements, this was very interesting. There were present—

1st. Cells of various forms.

2d. Peculiarly arranged fibrils (or mucin filaments?).

3d. Fibres having the appearance of fine elastic tissue.

These several elements would sometimes be mingled in one field of the microscope, but were more commonly found grouped separately, as if the tumour presented different degrees of development in different parts.

The cells were oval, round, caudate, bi- and tripolar, with one or two nuclei and bright nucleoli. Others had peculiar hernial dilatations, as if budding. Others again were apparently being changed into the forms resembling elastic tissue, already mentioned. None of the elements were affected by acetic acid except the cells, which were perhaps very slightly paled. This, however, may have been an optical effect from the action of the acetic acid upon the thin layer of mucin surrounding them.

The term enchondroma was first applied to tumours of this kind by Müller, and has reference to their resemblance to cartilage. They are when characteristic, firm, and sometimes hard and elastic. This latter quality has caused them to be mistaken for cysts, and under this impression they have been punctured. Their form approaches the oval.

The usual seats of their growth are in, upon, or near bones, most frequently the metacarpal bones and phalanges of the fingers. Next in frequency we find them affecting the glands, and of these most generally the parotid. They have been found in the testicle, mammary gland, and lung, and very rarely in the subcutaneous connective tissue. The one before us was, how-

ever, removed, as before mentioned, from the submucous tissue. This, perhaps, is its rarest locality; we have not as yet seen mention of them here.

Mr. Paget, in his admirable work on Surgical Pathology, page 432, describes a tumour with some few exceptions identical with the above. The one that he describes was removed from beneath the skin, near to the clavicle; there appeared to be no true cells, but nuclei among its elements.

A question of interest connected with this tumour is, whether its soft condition was a degeneration of a firmer tumour or a "defect" in growth. That it was the latter I am disposed to infer from the perfect condition of the cells and their abundance, as well as from the peculiar development of many of them.

It has been remarked by an eminent physician that much physiology might be learned by studying the pathological condition of the system. And we might add that much knowledge of the normal structures of the body may be obtained by the study of the abnormal. The pathological growths may present conditions allied to those found in the embryonic state, at which stage most light has frequently been thrown upon their development.

The close study of such tumours as the one before us would, no doubt, add to our knowledge of the development and connection between cartilage, white fibrous, and elastic tissue. I was struck, in the examination of this growth, with the peculiar fibrillation which radiated from a centre, the delicate fibres crossing each other in various directions. These were not connected with cell growth, but appeared as if produced by a species of crystallization from an amorphous blastema (mucin?).

Another striking element in this tumour was the peculiar elastic-like fibres and the cells apparently developing into these fibres, as seen in the figure. The question which arose was, whether the first-mentioned process was allied to the fibrillation of healthy cartilage from the hyaline structure, a beautiful instance of which is pictured in Prof. Leidy's work on Anatomy, page 176, and how far this might support Virchow's views as to the formation of the white fibrous tissue by a direct fibrillation of the blastema. Also, how far there was a connection between the cells of this tumour, which appeared to be elongating into elastic-like fibres, and the above author's ideas of its formation.

Feb. 27. *Anencephalous Monster, with Spina Bifida, and Failure of Development of the Anterior Abdominal Walls.* Dr. PACKARD exhibited this specimen, given to him without history, about eight years since.

It is a female child, born apparently at about eight months of utero-gestation. A membranous sac exists in connection with the head, forming the upper wall of the cranial cavity. No trace of brain matter can be detected, although the nerves may be seen passing through foramina in the base of the skull, in the eminences of which may be dimly traced the anterior and posterior clinoid processes, and the petrous portions of the temporal bones. There seems to be no frontal bone, except a thin border far back on the upper surface of the eyeballs. The rest of the vault of the cranium is reduced to a mere rim.

Another sac exists in connection with the cervical and dorsal portions of the spinal column; the pedicles of the vertebral arches are directed outwards, and the cord is represented by a bundle of fibres on either side, resting on the posterior surfaces of those processes.

The anterior wall of the abdomen was developed into a thin membranous sac, containing the heart and pericardium, and all the abdominal viscera.

Calcareous Degeneration of the Ovary.—Dr. HODGE exhibited a specimen of this lesion, with the following remarks:—

This specimen was presented to my father by Dr. Thornton, of Mississippi. It was removed from a negro woman, who died in her forty-fifth year. At the age of seventeen, she gave birth to a child, but never afterwards menstruated. She grew quite fat, and suffered no inconvenience from the tumour. The specimen is of an ovoid shape, measuring six inches and a half in its longitudinal diameter, and four inches transversely. It weighs one pound and twelve ounces. The surface is nodulated, very rough, and covered by a fibrous layer, which follows closely all its irregularities. The mass has been sawn open, and exhibits interiorly narrow, fibrous bands, running throughout the mass. The calcareous deposits in the interstices may possibly be the result of a degeneration of the Graafian vesicles.

Cancer(?) of Œsophagus.—Dr. KANE related the circumstances of an autopsy made by him, with the assistance of Drs. Mitchell and Packard, at St. Joseph's Hospital on the 24th February. The subject was an Irishman, æt. about 55, of large frame, admitted to the hospital a few days before his death; ascites, hæmatemesis, melæna, and exhaustion were his chief symptoms.

Belly largely distended, and fluctuating; about three gallons of yellow serum in the peritoneal cavity. Lower extremities œdematous. The *lungs* were healthy, with some *post-mortem* congestion. Adhesions of the pleura were observed posteriorly, especially on the left side; and bands of lymph connected the two layers of the left pleura at one portion opposite the middle of the pericardium. About a pint of serum was noticed in each pleural cavity. The pericardium was thickened by a deposit of fat. The *heart* was rather large and flabby; it had a good deal of fat upon its outer surface, but contained no clots; the tricuspid valve was healthy, one flap very slightly thickened; the pulmonary valves deep red, but not otherwise changed. The mitral valve was much thickened and stiffened; one of the chordæ tendineæ attached to it was thickened into a firm white mass. The aortic semilunar valves much thickened, and stiff from atheromatous deposit. The veins on the outside of the heart were quite tortuous, and felt firm under the finger. In the right ventricle a portion of the endocardium was reddened, as if by inflammation; in the left there were several patches of extravasation, one particularly marked upon a papillary muscle. The aorta presented a marked redness of its lining membrane, in some portions much deeper than in others.

The *liver* was much contracted, and in a state of complete cirrhosis. The *vena cava ascendens* was slightly bridged over by hepatic substance, and narrowed from its point of entrance below to its exit above, its cylinder being maintained by the firmness of the surrounding tissue. The vena portæ was of about the usual size and appearance, but its branches within the liver were diminished in calibre. The gall-bladder was full of yellow and extremely viscid bile, and its substance was very pale.

Pancreas large and quite firm, but nothing abnormal in its appearance on section. The capsule of the spleen was quite thick, and of a greenish or bluish-white colour; but the substance of the organ seemed absolutely healthy.

Kidneys and supra-renal capsules quite normal.

Near the lower end of the *œsophagus* was a long and wide irregular ulcer with frayed edges. Several veins were close to the surface of this ulcer, and one of them presented an open orifice, whence probably the fatal hemorrhage had occurred. The mucous glands of the tube were elsewhere much swollen.

Within the *stomach* there were f℥iv or so of soft blood-clots; the mucous membrane was mammillated, and along the rugæ were streaks of injection; but there was no ulceration at any point. The rest of the alimentary canal was in no respect abnormal, except in containing a grumous, bloody liquid, in not very large quantity. Blood throughout the body much less than normal, almost entirely free from clots. Much fat was present in the abdominal parietes, as well as in connection with the peritoneum covering the different viscera; the great omentum was very thick from fatty deposit.

Hypertrophy of the Pyloric Extremity of the Stomach.—Dr. Moss presented this specimen, with the following account:—

The subject of this lesion was a female æt. 52, a native of Alsace. The history of the case is very meagre, as Dr. Thompson, at whose request I made the autopsy, first saw her a week before her death. She had been ailing for several years, and had kept her bed for six months. She was excessively emaciated, and suffered from piercing pain in the epigastrium and from vomiting, soon but not immediately after eating. Very little if any blood appeared in the vomited matters. A hard nodulated tumour of considerable size was detected in the epigastrium; its position was not constant, changing into the left hypochondriac and umbilical regions. Three days before death the patient sank into a stupor, passing her urine and feces involuntarily. Several members of her family had died of phthisis; none had suffered from cancer.

Autopsy, thirty-six hours after death.—Cranium not opened.

Thorax, pleuræ, heart, and pericardium healthy, the latter containing about a wineglassful of fluid. The lungs were healthy, except at their apices, which were sparsely studded with hard cretaceous tubercles; over the left apex there was an old pleuritic adhesion.

Abdomen.—The position, size, and structure of the liver were unaltered. The stomach lay under the intestines across the spine; it was markedly enlarged, chiefly in its long axis, and its pyloric extremity was occupied by a hard tumour the size of the fist. A few glands in the vicinity were enlarged, and at its point of contact with the under surface of the liver that organ was rather more than commonly friable. The intestines and other abdominal organs were healthy. The stomach contained a small quantity of thin, dark-coloured fluid and a few lumps of vegetable ingesta. Its mucous surface was of a dull dirty white colour, and, except at the pyloric end, none of its coats were hypertrophied. The tumour entirely surrounded the pyloric end of the stomach, commencing abruptly at the pylorus, extending about four inches towards the cardiac end, and gradually decreasing in thickness, but at its junction with the healthy stomach an abrupt line could be felt. Its mucous coat was of the same colour as that of the rest of the stomach and perfectly intact, except at the lower and cardiac end, where there was an ulcer the size of a quarter dollar, which had destroyed the mucous coat and laid bare numerous transverse fibres in the tissue beneath; the surface of the ulcer was clean and hard, and so pale that it almost escaped detection. The tumour was hard and knobby, and very

tough under the scalpel when cut into. It presented to the eye two distinct layers; the outer one was translucent, of a yellow gelatinous colour, and firm to the touch, being evidently the muscular coat very much hypertrophied; in some places it was one-fourth of an inch thick. The inner layer, which was from one-half to three-fourths of an inch thick, was white, opaque, very tough, and sent off numerous fibres perpendicularly with the muscular coat.

Under the microscope.—The white abnormal structure proved to be composed of dense fibrous tissue at various stages of development, white and yellow fibrous tissue, fusiform cells, and roundish cells, the whole covered with fatty molecules. The outer translucent substance, in addition to these elements, was made up of unstriped muscular fibre. I was unable to discern any traces of cancerous structure or arrangement, either in the tumour or in a section taken from one of the neighbouring enlarged glands; I am therefore of opinion that this is a mere hypertrophy of the submucous areolar tissue and of the muscular coat, a lesion less frequently met with than cancer of the stomach, but probably frequently mistaken for it. The aspect of this tumour, the regularity of the arrangement of its elements, the want of fusion of its coats and of stenosis at the pylorus, tend to confirm this diagnosis.

Varicocele of the Leg.—Dr. BRINTON exhibited a specimen of injected varicose veins of the leg, removed from —— ——, an Irishman, æt. 55, who was admitted into the St. Joseph's Hospital with a compound fracture of the middle third of the tibia and fibula, the result of an accident from a passenger railway car. The leg was amputated at the upper third by Dr. McClellan some six hours after the reception of the injury. The hemorrhage after the operation was very abundant, necessitating the application of some fifteen or sixteen ligatures. Many of these were placed upon venous trunks which were discovered to be enormously dilated; their walls were thickened, and when cut across did not contract, but remained patulous. The bleeding was controlled with great difficulty, but by the removal of the tourniquet and the application of the ligatures was finally arrested. The patient did tolerably well for more than a week, despite a considerable oozing of venous character. On the tenth night, however, copious arterial hemorrhage occurred from the stump. This, it was found, could only be arrested by the ligature of the femoral artery, which was accordingly effected beneath the sartorius muscle. The ligature thus applied separated at the expiration of two weeks, and the patient sank from hemorrhage from the wound.

The veins of the leg were injected with wax by Dr. Brinton, a pipe being inserted into the internal saphena vein. The injection filled not only the superficial veins, but also ran into the venæ comites of the anterior tibial artery, which was found to be in an equally dilated condition. The fact of dilatation of the deep muscular veins being oftentimes coincident with spontaneous varicosity of the superficial veins of the lower extremity, has been insisted upon by M. Verneuil in a paper read before the Academy of Medicine at Paris, and the same occurrence has been observed by Dr. S. W. Gross of this city in a specimen recently exhibited to this society.

Dr. Brinton drew the attention of the members to the peculiar formation of fibrous tissue which had developed itself upon the concave portion of every one of the arches formed by the convolutions of the diseased vein. This tissue was so dense and hard as almost to defy removal without injury

to the walls of the vessel. The tenacity and firmness of this growth served apparently to limit the amount of the disease, and to restrain the natural tendency of the walls of the veins to yield at the summit of the convex arches.

A Remarkable Case of Congenital Aneurismal Varix of the Leg and Foot.—Dr. S. W. GROSS read the following case:—

An unmarried female, forty-three years of age, from Washington County, Pennsylvania, was admitted into the Hospital of the Jefferson Medical College, on the 10th of November, 1858, on account of a remarkable aneurismal enlargement of the superficial veins of the left foot and leg, from the base of the toes as high up as the knee. Not only were the vessels very tortuous and greatly dilated, but they were the seat of pulsation, synchronous with the contraction of the left ventricle, and perceptible both to the touch and to the naked eye. The aneurismal thrill was well-marked, and auscultation revealed a *bruit de souffle*, of a whiffing nature, and most distinct over the posterior tibial region. These signs were not so evident when the limb was elevated, but when it was dependent, the veins became larger, and the thrill and pulsation were much increased. The internal saphenous vein, which was particularly affected, appeared to be about an inch in diameter at various portions of its course, and the circumference of the leg was greater by an inch and a half than that of the sound one. The great and second toes were webbed, and a deep, excavated ulcer, as large as a ten-cent piece, with a foul bottom and everted edges, was seated in the dorsal surface of the foot, just in front of the two inner metatarso-pharyngeal articulations. The foot had a spongy feel, as if distended with enlarged veins, and it measured two inches more over the instep than the opposite one. The toes were distorted, and the leg, which was slightly longer than the right, was at times the seat of great pain. The ulcer occasionally bled, and always healed when the limb was kept elevated and at rest, but as it was impossible to observe these precautions, it has remained open for a number of years.

The patient was born with a nævus maternus, which was situated on the back of the foot, between the instep and the toes, and at the age of three years the foot began to enlarge, the superficial veins and arteries became tortuous and increased in size, and the temperature of the member was elevated. Six years subsequently the ulcer made its appearance near the web of the great toe, and about four years previous to her admission into the hospital, sores broke out on different portions of the foot, the largest being seated over the internal malleolus. As these bled freely whenever she walked, it was necessary for her to keep at perfect rest for eighteen months. She had occasional palpitation of the heart, but there was no evidence of disease of that organ, nor was there ever any arterial disease in her family. Her general health was pretty good up to 1856, during which year she suffered from an attack of bilious remittent and of typhoid fever. Her hair was brown, her eyelashes long, and her complexion dark; but her face presented a sallow, doughy appearance. The pulse was seventy-two, and she felt and looked rather well. Seventeen years ago a physician took up two arteries on the inner and upper portion of the leg, and failed in an attempt to ligate the femoral in the lower part of its course.

As the leg was the seat of a great deal of pain, and the patient was obliged to use crutches, from inability to bear her weight on the foot, the limb was utterly useless, and a source of inconvenience. On this account

it was determined, without resorting to other curative measures, to remove it. The amputation was accordingly performed by Professor Gross, below the knee, by the antero-posterior flap method, on the 13th of November. The internal saphenous vein emitted a good deal of blood, and as it behaved like an artery, not contracting when divided, and the orifice being at least three-fourths of an inch in diameter, it required the application of a ligature. At the expiration of sixteen days all the ligatures had come away, and the wound of the stump had nearly closed. The patient had taken a nutritious diet, with tonics and opiates from the very commencement of the treatment, but the stump discharged a considerable quantity of slightly offensive and badly elaborated pus. On the eighteenth day the integument of the popliteal region had an erysipelatous appearance, and distinct fluctuation being present, an opening was made, with the effect of giving issue to about six ounces of thin, unhealthy, fetid pus. The stump was painful and tender on handling, the pulse frequent and irritable, the countenance pale and anxious, and the patient was much depressed in spirits, and very restless. About eighteen hours later chilly sensations set in, followed by increased heat of the surface, and profuse perspiration; the features became shrunken, the skin icterode, the body emaciated, and hectic fever with typhoid symptoms rapidly supervened. The discharge of pus became so profuse as to require the limb to be dressed morning and evening; it was excessively fetid and nauseating, and at each dressing at least a pint of matter was pressed out of the opening made in the ham. A large bedsore formed over the sacrum, and the patient lingered in a low, muttering delirium until the twenty-eighth day, when she died in a state of complete exhaustion.

The treatment adopted in this case consisted in the free administration of quinine, the tincture of the chloride of iron and ammonia, with a nourishing diet and an abundance of milk-punch. Opiates were given freely to relieve pain and procure sleep, and the stump was enveloped in warm dressings, and injected with the dilute chlorinates, which were also freely employed externally. Tincture of iodine was applied to the limb as far up as the pelvis, and the thigh was also supported by a roller, for the purpose of facilitating the escape of pus, and to prevent it extending higher up. The apartment was ventilated as thoroughly as possible three or four times in the twenty-four hours.

A *post-mortem* examination, made about sixteen hours after death, had to be limited to an inspection of the limb. On cutting into the thigh, it was found to be the seat of an immense diffuse abscess, extending from the stump to the crest of the ilium, and dissecting the softened muscles from each other. The popliteal artery and vein were bathed in a depot of pus, which was thin and offensive, of a grayish colour, and mixed with aplastic lymph. The internal saphenous and popliteal veins were plugged with clots about six inches in length, and contained sanious pus and broken down lymph; their lining membrane was inflamed, and their coats thickened.

The amputated limb was injected with a mixture of tallow and magnesia through the posterior tibial artery, and the material flowed not only into the superficial and deep veins and arteries, but escaped very abundantly from the medullary canals of the tibia and fibula. Upon inspecting the dried and prepared specimen, it will be found that all the superficial veins are varicose to a most striking extent, as are also the deep veins, but in a lesser degree. At many points the internal saphenous vein is dilated to

the extent of three-fourths of an inch in diameter, its course is tortuous, and its coats are greatly thickened. The veins emptying into it are much enlarged and twisted, and they form an intricate plexus over the dorsum of the foot, and the anterior and internal aspects of the leg. The dissection of the specimen was somewhat difficult from interstitial deposits of lymph, and it was impossible to bring into view the enlarged veins and arteries on the plantar surface of the foot, which formed a most intricate network, or rather a confused mass of injecting material. The external saphenous vein is slightly enlarged, and is accompanied by several smaller varicose vessels. The posterior tibial artery is nearly as large as the femoral, and its accompanying veins are twisted and nodulated.

Remarks.—Aneurismal varix is most frequently observed at the bend of the elbow, as the result of badly performed venesection; but it may also be of spontaneous origin, on account of ulcerative action taking place between a contiguous artery and vein. In either event the arterial blood entering the vein communicates to it a pulsatile movement, synchronous with the contraction of the left ventricle, and the consequence is that the resistance of the walls of the veins being weakened, it becomes dilated, as in the case of ordinary varix. In the instance, however, before us—and I can find none of a precisely similar nature on record—the great point of interest is that the aneurismal varix is not due to either of the above-mentioned causes; but its origin can be traced to a congenital nævus maternus seated on the dorsum of the left foot below the instep. In the first place, then, there was a small spot, consisting of slightly enlarged capillary arteries and veins, which finally became enormously hypertrophied and dilated. The tumour may be said to be situated on the back and sole of the foot where the capillary enlargement is best marked, and the arteries and veins at some distance beyond its limits greatly increased in size and tortuous. From the fact that the posterior tibial artery is in a state of cirsoid dilatation, it is probable that the great bulk of the swelling on the sole of the foot is due to the enlargement of the terminal capillaries of that trunk. Inasmuch as it was impossible to distinguish any artery, excepting the posterior tibial, on account of the free manner in which the injection ran, I have represented the enlarged vessels as veins, by giving them a blue colour. It is interesting to note that the symptoms of purulent infection came on within forty-eight hours after the detachment of the ligatures from the arteries of the stump and the internal saphenous vein; and the general condition of the patient, as well as the unhealthy character of the pus discharged from the stump, pointed to the existence of a pyogenic or suppurative diathesis.

March 27. *Monstrosity.*—Dr. PACKARD exhibited a malformed fœtus, obtained by him at the University dissecting-room, whither it had been sent without history.

It would seem to have been born at full term, although of small size. Except the left foot, which presented a marked talipes varus, the extremities were perfect, as were also the bony portions of the trunk. The head was in all respects well formed.

Just below the point of entrance and exit of the umbilical vessels, the skin of the abdomen became continuous with the outer covering of a finger-like protrusion, about one and a half inch in length; this outer covering had the valvulæ conniventes, and the vascular, villous appearance of the mucous coat of the jejunum, which, indeed, it proved to be. At the tip of the protrusion there was an orifice, the edges of which were formed by the

inversion of the mucous and other coats of the tube, through which a probe could be readily passed into the upper part of the small intestine. Hence there may be said to have been an artificial anus at the umbilicus, with a prolapsus of the intestine.

Below the protrusion just mentioned, there was an irregular swelling, at the lower part of which an opening led into a tube one and a half inch long, the direction of which was downwards and backwards in a curve, but keeping the median line. This tube was lined, and the swelling at its upper end was covered, with smooth red mucous membrane; it terminated in a blind pouch, and evidently represented the lower portion of the colon, a fibrous cord continuous with it indicating an imperforate rectum.

At either side of this tube, firmly attached by areolar tissue to the brim of the pelvis, lay a somewhat irregularly bean-shaped body. Being laid open, each of these was seen to consist of a uterus, with a well-formed os tincæ, and a very short vagina; this latter had on the right side an external opening, while on the left it was closed, perhaps by mucus. Connected with the upper end of each of these bodies was a distinctly marked ovary and corpus fimbriatum; so that the abnormity here seemed to be merely a wide separation of the two symmetrical portions of the uterus, and not a duplicity of the organ.

No urinary bladder existed; but the ureters descended normally, and each one ran under the corresponding vagina, to terminate at its outer side. Thus the intervening cutaneous surface represented an exstrophied bladder. Just within each inguinal fold was a small prominence of skin, doubtless the indication of the labia majora; no other distinct trace of external organs existed.

Posteriorly, at the lower extremity of the back, the skin was elevated into a rounded swelling over a sac communicating with the dura mater, and containing f℥j or more of bloody serum. This sac seemed to be simply supplementary to the spinal canal; at its upper part, on its anterior surface, were two rounded vascular masses of a tufted appearance.

It was easy to perceive that there was a deficiency in the junction of the lateral halves of the pelvis anteriorly; and dissection showed that this was owing not to the absence of any part of the bony walls, but simply to a separation of the bodies of the pubic bones, the interval between them being filled up with areolar tissue and fat. It need hardly be suggested that this separation and that of the lateral halves of the uterus were intimately connected, and probably due to the same cause, whatever that may have been.

Spina Bifida, with Cyanosis.—Dr. KANE gave the following account of the case, exhibiting the parts:—

The subject from which these specimens were obtained was born under the auspices of a student in Dr. Penrose's obstetrical class, and the history of the case is unfortunately meagre and unsatisfactory. The mother, a delicate florid-complexioned woman of about twenty years of age, was, during the first six months of her pregnancy, under my care for disease of the heart, with mitral regurgitation, accompanied by severe pains in the back, and a constantly recurring menstrual flux; the latter symptom came on about eighteen months before, shortly after the birth of her first child, and continued at intervals of two weeks until within four months of her last confinement. About three weeks before this event she experienced severe pains, and had a slight discharge from the vagina; but the symptoms passed off. The labour was normal and easy.

The child, which was a male, cried freely on entering the world, and its functions were naturally performed. The region of the sacrum was unusually flat, and covered only by a thin transparent pellicle, through which the vessels and moving fluid could be plainly distinguished. The following morning this pellicle had developed into a fluctuating tumour; on this morning, also, it was observed that the child appeared to have an impediment in his breathing, and that he sucked with difficulty. The latter symptom continued to increase until the 15th of February, 1861, when death took place.

The tumour was at first pellucid, but it shortly assumed a dark purple colour. It slowly increased in size during thirteen days, when it burst and evacuated its entire contents, which consisted mainly of bloody serum with some pus. It scabbed over and gradually became distended again, but never entirely regained its former size.

I first saw the child on the afternoon of February 15th; it was then in a dying condition. It was lying perfectly still in its mother's lap with its eyes closed, breathing very slowly and apparently with great difficulty. Several seconds invariably elapsed between the inspirations, which were made with short spasmodic jerks, and were accompanied by a whooping sound and by a depression of the trachea. I could not detect any corresponding depression of the intercostal spaces or abdominal muscles, although the thorax appeared to expand but little. The entire surface of the body was of a dark crimson hue.

Percussion elicited a clear sound over the entire chest. My efforts at auscultation were unsatisfactory, owing to the jerking character of the respiration and the position of the child in its mother's lap, any attempt at altering which was attended by an entire cessation of respiration for from fifteen to twenty seconds, during which time the child would become so livid that I feared immediate death.

The fontanelles were large, and the separation of the cranial bones at the sutures rather wider than usual.

The sacral portion of the spine was the seat of a fluctuating, semi-transparent, lobulated tumour about the size of a green-gage, constricted at the base, and of a dark purple colour; its tegumentary covering was very thin, and ulcerated over its most prominent portion. This tumour was evidently connected with the medullary canal; by pressing near its constricted margin I could readily feel the free margins of the imperfectly developed half arches of the sacral vertebræ. The rest of the spinal column appeared to be normally developed, all the spines being prominent. The left leg was partially paralyzed, and presented the somewhat rare deformity known as talipes calcaneus. The child remained in the condition I have described until noon of the following day, when it died. There was no convulsion preceding death.

Autopsy twenty-three hours after death.—The body was rather small, but not emaciated. There was marked rigor mortis.

The lungs floated on water, but were much congested, especially at their upper portions, the congestion being most marked at the apex of the left lung.

The heart was about the usual size and shape, and all its valves were normally developed, but the ductus arteriosus and the foramen ovale remained pervious. All the cavities of the organ were distended by soft dark clots.

The liver, kidneys, and intestines were normal.

The brain was not examined.

The fluctuating tumour over the sacral region of the spine had somewhat diminished in size, not being larger than a large prune-plum. On cutting through its outer walls, which were very thin and appeared to be composed of the integuments and dura mater, about two teaspoonfuls of bloody serum escaped, bringing to view an inner sac, much smaller, which protruded from the spinal canal through the deficient half-arches of the sacrum. This I took to be the true arachnoid sac; it was about an inch in length, and as large round as a large swan-quill. When first exposed, it was filled with a clear pellucid fluid, but gradually emptied itself. On laying it open, several filaments of nerves were seen losing themselves upon its inner surface. The sacral half-arches were entirely wanting, and those of the fifth sacral vertebra were only partially developed, the union between them being formed by a ligamentous band. The rest of the spine, however, was normally developed.

Members of the family who remained in the room objected to my proceeding further with my examination, so that I was unable either to examine the brain and upper cervical vertebræ, or to trace out the sacral nerves.

April 10. *Abscess in the Head of the Tibia.*—Dr. HODGE exhibited a diseased tibia, with the following remarks:—

C. D., a coloured woman, sixty-four years of age, was admitted to the Pennsylvania Hospital, May 3, 1859. She stated that for about forty years she had had more or less trouble with her left leg. At various times ulcers had formed upon it, but they had successively healed. During the winter of 1856–7, she had been in the hospital for these ulcers. Since then she had been moving about as usual, except during the last few months.

At the time of her admission, the whole leg was much swollen; there were two ulcers above the internal malleolus, and in front, just below the knee, a sinus leading to dead bone, with two fistulous orifices on the inner side of the knee. The pus discharged was in very great quantity, and of an offensive odour. Her system was very much weakened; she suffered a great deal of pain, and could obtain but little sleep.

After consultation with his colleagues, Dr. Neill amputated the limb just above the knee. Mortification of the flaps ensued, and in a few days the woman died.

The head of the tibia presents a circumscribed abscess measuring in its length two and a quarter inches, and in its width varying from three-quarters to one and a half inch. Its depth is about one inch, and its external orifice is about a quarter of an inch in diameter, and smoothly rounded. The bone around the abscess is hypertrophied and roughened. The periosteum was much thickened. The orifice of the abscess is in front, and about one and a half inch below the knee.

April 24. *Secondary Carcinoma.*—Dr. HODGE related the case, as follows:—

On the 9th of November, 1859, I presented to the society a specimen of cancer of the breast removed that morning by Dr. Norris. (See *Proceedings of Path. Soc.*, vol. i. pp. 268–270.) The wound made in its removal healed kindly. About eight weeks, however, after the operation, little nodules of characteristic hardness were detected a little way above the scar. They gradually increased in size, and the patient soon experienced a return of her old lancinating pains. Her appetite and strength began to fail, so that by May she could only walk a few squares. At that time she was liable to

severe attacks of pain in the breast, and also experienced a constant sensation of soreness. The hardened nodules varied in size from that of a pin's head, just beneath the skin, to that of a hickory-nut, evidently involving the deeper seated tissues. In front towards the sternum, and behind towards the axilla, these nodules had ulcerated superficially. Under the simplest local applications (lime-water almost exclusively) these ulcers rapidly healed, the nodules gradually diminished and disappeared, leaving the surface perfectly even and the skin sound, though very vascular and delicate, till she died. In the meanwhile, the cervical glands of the same side (the left) became enlarged, then also those on the opposite side, and last fall I noticed that a gland of the right axilla became affected. Soon the right breast, which up to this time had been large and pendulous, began to contract and to harden. During the last month or so of her life, it was of a stony hardness, perfectly rigid, and about the size of half of an orange. It never caused her any pain, and but little inconvenience, excepting the nipple, which became very sensitive to the touch, apparently from the constriction at its base caused by the great condensation of the tissue of the breast. Little hardened nodules constantly appeared in the skin, especially on the lower and anterior portion of the thorax. At one time these caused her intense suffering for a day or two, but ceased to annoy her after the application of some glycerin. The greatest pain that she experienced, however, was seated in her left arm. Soon after the return of the cancer to the breast this arm began to swell, and continued enormously swollen until she died. Its weight caused her great inconvenience at all times, and the occurrence of lancinating pains throughout it were frequent.

Her general health gradually failed her, though she never seemed to despond. In December last she began to vomit nearly everything she ate, and was constantly oppressed with nausea. This, however, somewhat improved, and for a time she relished some few things. In February she was troubled by a severe cough, free expectoration, and great dyspnœa. These symptoms also improved, but left her much weaker, and with a sense of weight about the region of the heart, and difficulty in breathing. Throughout the chest there was great dulness on percussion and obscurity of the respiratory murmur. During her illness her bowels were very costive. Her catamenia returned regularly until January, when they ceased to appear. During the last few weeks of her life she was liable to "screaming spells," as her friends called them; she was always conscious that she had screamed, but never gave any reason for doing so. At times she would talk strangely, but still knew everything around her. She daily grew weaker, and on Sunday, April 21st, she quietly died.

Upon making the *post-mortem examination*, the *brain and its membranes* were found deeply congested, but exhibited no marks of cancerous degeneration. There was great effusion of serum into both *pleural cavities*, especially the left. The *left lung* was collapsed, adherent to the walls of the chest, and filled with cancerous deposits. The *right lung*, though in a much less degree, was similarly affected. The *pericardial cavity* was filled with serum, and its tissue, especially towards the left side, was studded with cancerous nodules. The *stomach* exhibited no evidence of cancer. The *liver* had three or four white cancerous deposits in it about the size, severally, of a half-dime. The *kidneys* exhibited a few cancerous particles. The *mesenteric glands and the ovaries* were perfectly filled with cancer. The *uterus* appeared to be unaffected. The *right breast* also was cancerous. The *skin*, and all the *soft tissues* of the thorax and abdomen were

filled with the cancerous deposits so as to have lost their elasticity. We were unable to examine the left arm.

June 12. *Cancerous Tumour of Cerebrum.*—Dr. HODGE related the following case:—

In August, 1858, Miss H., about forty-eight years of age, noticed a small lump in her right breast. During the fall and winter, she says that the tumour continued to enlarge, and at times caused her severe lancinating pain. In April, 1859, she was admitted into the Pennsylvania Hospital. The tumour was quite prominent, the skin was of a purplish hue over the upper and posterior surface of the breast, and seemed on the point of giving way. After a careful examination, the surgeons of the House pronounced it cancerous in its nature, and advised its removal. The microscope afterwards corroborated their opinion. The patient's general health had suffered much during the winter, she had lost flesh, and, always of an excitable temperament, had now become excessively nervous.

On the 10th of April, 1859, Dr. Neill excised the tumour and the mammary gland. The wound readily healed, and the patient rapidly improved in health and strength. She became very fat and more robust than she had, perhaps, ever been before. Last January, however, her catamenia ceased to appear, and she experienced, at irregular intervals, flushes of heat in her head. This did not amount to much until April; she sent for me on the 12th, continued about the same until the 17th, when she became a little confused in her mind, and about mid-day had a clonic convulsion, beginning with her right arm, but extending to her whole body. A little indisposition to use her right arm followed this attack, but in ten days she seemed almost to have recovered herself. Then, however, her mind became somewhat dull, her memory often failed her, and her right arm became totally paralyzed; this was followed by a paralysis of her right leg. At times she would for a few days improve, but she gradually became more stupid, and died on the 4th of June.

Upon making the *post-mortem* examination we found no appearance of cancer about the breast, but a cancerous tumour in the cerebrum. It was situated in the left hemisphere at the anterior margin of the lateral ventricle. Its size was about that of an English walnut. The brain around it was softened, and all the tissues of the cerebrum and cerebellum were much congested. There was no effusion except into the left ventricle, which was distended with serum. Every other organ of the body was perfectly normal, except the uterus, which was studded, both within and without, with fibrous deposits of various sizes.

REVIEWS.

ART. XI.—*The Pathology and Treatment of Venereal Diseases; including the Results of Recent Investigations on the Subject.* By FREEMAN J. BUMSTEAD, M. D., Lecturer on Venereal Diseases at the College of Physicians and Surgeons, New York; Surgeon to St. Luke's Hospital; Assistant Surgeon to the New York Eye Infirmary. Philadelphia: Blanchard and Lea, 1861. 8vo. pp. 686.

THE perusal of this work has afforded us unfeigned satisfaction, and we conceive that we are simply fulfilling a duty in inviting to it the attention of the profession. The previous labours of the author afford evidence that he possesses an extensive and accurate acquaintance with the literature of venereal diseases, and that he has also had a large experience in their treatment, but something more than this is requisite for the preparation of a good work on these diseases. An extensive acquaintance with the literature of his subject often leads a writer to make practical details subordinate to theoretical discussions, and practical knowledge of the treatment of a disease is by no means inconsistent with a total inability to describe clearly the manner in which this treatment is to be successfully executed. Moreover, an advantageous arrangement of the various subjects, and a correct style, are equally essential, and are more frequently wanting, perhaps, in works on venereal affections, than in any others offered to the medical profession. The work before us, however, we find in every respect—in matter, in arrangement, and in style—all that could be desired.

A work, such as this, on venereal diseases, was greatly needed. The doctrines taught in the special treatises on the subject, and in works on general surgery are always incomplete, often contradictory, and very generally erroneous. The treatment practised by the profession at large is, as a matter of course, far from being judicious. Under the belief that mercury is absolutely necessary in the treatment of syphilis—acquainted with no means of diagnosing this disease—and believing it best to be on what is considered the safe side, medical men are often seen administering this powerful medicine for every running from the urethra, and for every sore spot upon the penis. We were lately advised by a practitioner of very considerable eminence, that John Hunter was the only writer on syphilis who ought to be followed, and that all venereal sores should be treated in accordance with his directions. A large number have not yet reached the epoch of Hunter, and give mercury not only in all forms of venereal ulcer, but also in every case of urethral discharge. And this is done in accordance with what is taught in books printed at the present day. At the same time, while no diseases are so imperfectly understood by the profession at large, and so incompletely and incorrectly treated of in medical works, there are none that cause more physical and moral suffering, directly or indirectly, to the patients themselves—*animasque in vulnere ponunt.*

Within the last ten years many and most important additions have been made to our knowledge of the pathology and treatment of venereal diseases—

additions which render plain what was before obscure and inexplicable, and which enable the surgeon to diagnose these affections with almost absolute certainty, and to pursue their treatment with confidence and satisfaction. Much has been learned, too, in regard to their history, and it is very curious to see how, after the expiration of so many years of discussion and experimental research, we are led to adopt the very identical opinions, in regard to the pathology of these diseases, that were universally held three centuries and a half ago. The distinction that is at the present day admitted between the three diseases which, from their origin in sexual intercourse, have been denominated venereal, namely, gonorrhœa, the contagious ulcer of the genitals, or chancroid, and syphilis, was fully recognized and taught at the beginning of the sixteenth century.

Among the most important of the recent additions to our knowledge of venereal diseases may be mentioned the distinct nature of the two species of chancre; the innocuousness of the secretion of the infecting chancre when applied to the person bearing it, or to any individual affected with the syphilitic diathesis; the removal of certain obstacles to a general belief in the contagiousness of secondary lesions; the fact that syphilis pursues the same course whether derived from a primary or secondary symptom, commencing in either case with a chancre at the point where the virus enters the system; the definite period of inoculation of the true chancre, and of general manifestations; the inefficiency of the abortive treatment of syphilis; and the phenomena of syphilization and their correct interpretation.

These additions to our knowledge of venereal diseases, with the investigations which have led to them, are scattered through the pages of medical periodicals, or contained in lengthy monographs devoted to one particular point in dispute; and a full and comprehensive treatise on these affections, written in accordance with the doctrines now received, is the book, at the present day, most needed in medical literature.

Dr. Bumstead's work furnishes the student with a full and comprehensive treatise upon venereal diseases, and also the practitioner with a plain and practical guide to their treatment. Knowing from his own experience, which has evidently been very considerable, that the success of treatment depends quite as much upon the manner of its execution as upon the general principles upon which it is based, he leaves nothing unnoticed that is calculated to assist the surgeon, or benefit the patient.

After an Introduction, containing a very interesting and instructive account of the changes in medical belief in regard to syphilis, the present volume is divided into two parts; the first devoted to gonorrhœa and its complications, and the second including both the chancroid and its complications, and syphilis.

The first part is divided into thirteen chapters. Each of the complications of gonorrhœa, swelled testicle, inflammation of the prostate, inflammation of the bladder, gonorrhœal rheumatism, &c. &c., is treated of in a separate chapter, and in the most thorough and satisfactory manner. The surgeon will find here all that he need desire to know in regard to these troublesome affections, both of their pathology and their treatment.

From his own observations, Dr. Bumstead is convinced that gonorrhœa is often due to accidental causes, and not to direct contagion. He has no doubt that it does at times originate in coitus just before, after, or during the menstrual period, or with a woman suffering from leucorrhœa, and, in a few instances, when nothing whatever abnormal can be discovered in the female genital organs, and the disease in the male can only be attributed to

the irritant character of the vaginal or uterine secretions (p. 46). It is greatly to be regretted that this opinion, which accords with that of Ricord, Diday, and the best informed authorities upon the subject, is not universally held by medical men. The maintenance of a contrary opinion has repeatedly been the cause of great distress, and has even caused suicide and death.

The "abortive treatment" of gonorrhœa, Dr. Bumstead believes to be a highly valuable method of treatment, unattended with danger, and undeserving the censure sometimes cast upon it. It may, however, produce most unpleasant consequences, if not used with proper caution, and if not limited to the early stage of gonorrhœa, when the discharge is but slight, and chiefly mucus, and while as yet there is no severe scalding in passing water. The weak injection (one grain of nitrate of silver to six ounces of water), repeated every three hours until a sufficient effect is manifested, is much preferred by Dr. Bumstead to the strong one usually made use of. The directions given in regard to the cases in which the abortive treatment can be employed, and the manner in which the injections are to be practised, are excellent. As an example of this we will cite the directions for their administration, and this description of the method of using the syringe is, in the main, applicable to all the injections which may be required in the course of a gonorrhœa.

"The patient should be made to pass his water immediately before injecting, or, better still, a quarter of an hour before. We wish to clear the urethra of matter, and to have the bladder empty, so that the injection may have some time to act before it is washed away by another passage of the urine, and yet a short interval between the last act of micturition and the injection is advisable, in order that as much of the urine as possible may have drained from the canal, and little be left to decompose the nitrate of silver. The prepuce should now be fully retracted, and the glans penis exposed. The latter should be wiped dry, so as to afford a firm hold to the thumb and forefinger of the left hand, applied to its opposite sides, and firmly compressing it around the point of the syringe introduced to its full extent within the meatus. If this pressure be properly made, not a drop of the solution will be lost, as the piston of the syringe is slowly forced down by the forefinger of the right hand holding the instrument, and the whole contents will be discharged into the canal. The syringe should now be withdrawn, and the fluid still retained for a few seconds by continuing the compression of the glans. When the injection is allowed to escape, it will be found to be of a milk-white colour. This is due to the partial decomposition of the contained salt by the remains of the urine and the muco-pus in the canal. As this decomposition has prevented the application of the injection in its full strength to the urethral walls, a second syringe-full should be thrown in, and retained for two or three minutes. During this time, a finger of the disengaged hand should be run along the under surface of the penis *from behind forwards*, so as to distend the portion of the canal occupied by the injection, and insure the thorough application of the fluid to the whole mucous surface." (pp. 57–8.)

For throwing injections into the urethra, the surgeon will find great advantage in using the syringes now manufactured from hardened caoutchouc. In these instruments, the diameter of the cylinder is in all parts the same; the piston works with great accuracy; the material is not acted upon by ordinary medicinal agents, and the different sizes and forms of the instrument are adapted to the various purposes for which it is required. Glass syringes are apt to be unequal in caliber in different parts of the cylinder, and the wadding of the piston contracts in drying, so that their use is attended with much annoyance, and the treatment seriously interfered with. In many cases of gleet the deeper seated portions of the urethral canal are involved, and for the purpose of reaching them with injections the "urethral syringe with extra long pipe," manufactured of hardened *rubber*, will be

found particularly valuable. The long pipe may be bent to any curve desired, by first heating it over a spirit-lamp.

Part II. contains the best account of syphilis and of chancroid that is to be found in the English language, and it is, moreover, the only one that is fully in accordance with the results of recent investigations and discoveries. We doubt whether in any language so clear, thorough, and satisfactory a treatise on these diseases can be found, and it is very seldom we meet with a treatise on any subject that can equal this in style, methodical arrangement, closeness of reasoning, and full acquaintance with the facts concerned.

This part of the work is divided into eighteen chapters, devoted to Introductory remarks; Chancres; Affections of the lymphatic vessels and ganglia attendant upon primary sores; General syphilis; Treatment of syphilis; Syphilitic fever, state of the blood, and engorgement of the lymphatic ganglia; Syphilitic affections of the skin; Syphilitic affections of the appendages of the skin; Mucous patches; Gummy tumours; Syphilitic affections of mucous membranes; Syphilitic affections of the eyes; Syphilitic affections of the ear; Syphilitic orchitis; Syphilitic affections of the muscles and tendons; Syphilitic affections of the nervous system; Syphilitic affections of the periosteum and bones; and Congenital syphilis.

We conceive it impossible for any one to read the chapter entitled "Introductory Remarks," without being convinced that there are two species of chancre; one, a simple chancre, limited in its action to the neighbourhood of its site; the other, an infecting chancre followed by constitutional manifestations. Their duality is established upon the same evidence as that by which naturalists determine the identity of species in the animal and vegetable kingdoms; namely, by the immutability of certain traits in successive generations. Though the virus from both is called *chancrous*, there is but one *syphilitic* virus. Syphilis implies not only a local, but a general disease; conditions which are fulfilled only by the hard chancre and its consequent constitutional symptoms. The soft chancre, and its attendant bubo, should not, properly speaking, be described under the head of syphilis, but be considered apart like gonorrhœa, and though we venture to differ from Dr. Bumstead with great hesitation, we should have preferred not to see them described together in his work. Considered apart in medical works there would be less danger of their being confounded in practice.

The diagnosis of the two species of chancrous sore is, of course, a matter of the highest importance; the one being merely a local affection, while the other more powerfully modifies the constitution for the remainder of life than any known disease, and exercises a greater influence upon posterity. Their diagnostic characters are well set forth by Dr. Bumstead, as follows:—

THE SIMPLE CHANCRE.	THE INFECTING CHANCRE.
Origin.	*Origin.*
Always derived from a simple chancre, or virulent bubo.	Always derived from an infecting chancre or secondary lesion.
Anatomical characters.	*Anatomical characters.*
Generally multiple, either from the first or by successive inoculation.	Generally simple; multiple, if at all, from the first; rarely, if ever, by successive inoculation.
An excavated ulcer, perforating the whole thickness of the skin or mucous membrane.	Frequently a superficial erosion; not involving the whole thickness of the skin or mucous membrane, of a red colour, and nearly on a level with the surrounding surface. Sometimes an ulcer, when its

Edges abrupt and well-defined, as if cut with a punch, not adhering closely to subjacent tissues.	Edges are sloping, hard, often elevated, and adhere closely to subjacent tissues.
Surface flat but uneven, "worm-eaten," wholly covered with grayish secretion.	Surface hollowed or scooped out, smooth, sometimes grayish at the centre.
No induration of base unless caused by caustic or other irritant, or by simple inflammation; in which case the engorgement is not circumscribed, shades off into surrounding tissues, and is of temporary duration.	Induration firm, cartilaginous, circumscribed, movable upon tissues beneath. Sometimes resembles a layer of parchment lining the sore. Generally persistent for a long period.
Pathological tendencies.	*Pathological tendencies.*
Secretion copious and purulent, inoculable.	Secretion scanty, chiefly serous; inoculable with great difficulty, if at all, upon the patient or upon any person under the syphilitic diathesis.
Slow in healing. Often spreads and takes on phagedenic action.	Less indolent than the chancroid. Phagedena rarely supervenes and is generally limited.
May affect the same person an indefinite number of times.	One attack affords complete or partial protection against a second.
Characteristic gland affections.	*Characteristic gland affection.*
Ganglionic reaction absent in the majority of cases. When present, one gland acutely inflamed and generally suppurates. Pus often inoculable, producing a soft chancre.	All the superficial inguinal ganglia, on one or both sides, enlarged and indurated; distinct from each other, freely movable; painless, and rarely suppurate. Pus never inoculable.
Prognosis.	*Prognosis.*
Always a local affection, and cannot infect the system. "Specific" treatment by mercury and iodine always useless, and, in most cases, injurious.	A constitutional affection. Secondary symptoms, unless prevented or retarded by treatment, declare themselves in about six weeks from the appearance of the sore, and very rarely delay longer than three months.

Of course, while holding these opinions in regard to the pathology of the two species of chancre, Dr. Bumstead teaches that in their treatment, the internal use of mercury is justifiable only in undoubted cases of infecting chancre. When there is doubt as to the nature of a chancre, it should be treated as belonging to the simple species, and the patient should be kept under observation until the period of incubation of secondary symptoms has been passed in safety. A chancre which is not subjected to specific treatment, will generally, if at all, be followed by secondary symptoms within three, and at farthest within six months.

It is well determined at the present day that a considerable length of time may intervene between exposure and the appearance of the infecting chancre. One of the most conclusive cases we have yet met with, in evidence of this, is given by Dr. Bumstead at page 370–1. In this case it was not until the thirty-fifth day after exposure that a slight excoriation was noticed, though the patient had examined himself daily with the greatest care to see if he had contracted any disease. We have ourselves, at the present moment, a young unmarried man under care with an indurated chancre and induration of the glands in the groin; and from what he relates, and from what we believe, it was six weeks after sexual connection that he observed the first appearance of the sore, which is situated upon the external surface of the prepuce, on the upper part of the penis, where it could not have existed unobserved.

As regards the doctrine of the abortive treatment of chancres, Dr. Bumstead adduces a number of facts to show that destructive cauterization of

an infecting chancre at a very early period of its existence does not prevent secondary symptoms, and hence that the system must be regarded as infected from the very first. In other infectious diseases, as vaccinia, glanders, etc., the absorption of the virus is instantaneous, and analogy would lead us to infer the same to be true of syphilitic virus. But although this argument from analogy is of very considerable weight, it is not conclusive, and the question can only be settled by facts. From cases recorded it must at the least be concluded that destructive cauterization *a few hours after the development of the chancre* has failed to avert constitutional infection, and that the abortive method cannot be relied upon.

In the chapter on General Syphilis, the question of the contagiousness of certain secondary symptoms will be found most ably discussed. The amount of evidence now brought forth to prove their contagiousness is so great that it can scarcely be doubted any longer; and we are ourselves as fully convinced of the transmission under certain circumstances of infection from a secondary sore as of any other fact in medicine.[1] Whether derived from a primary or secondary symptom, syphilis pursues essentially the same course; in the latter case, as in the former, the initial lesion is a chancre.

We heartily congratulate the profession on the possession of this treatise on venereal diseases, and we would also earnestly urge all practitioners, for their own pleasure and profit, and, more than this, for the great benefit of their patients, to avail themselves of the opportunity thus offered of making themselves acquainted with the important facts recently discovered in regard to the pathology and treatment of these affections. A very wise man who wrote nearly three centuries ago tells us that it is of great importance to have regard to the age in which one writes—"*il importe beaucoup de regarder en quel age on escrit.*"

The writer of the volume to which we have just called attention has the rare advantage of having acted in accordance with this advice in every respect; his work is fully up to the advanced stage of our science, and it is likewise the work which was most needed at the present time by the profession.

In the getting up of this volume the publishers have made the best use of the great facilities at their command, and have omitted nothing in the way of paper, letterpress, or illustrations, that could add to its appearance or its usefulness. W. F. A.

ART. XII.—*A Course of Lectures on the Physiology and Pathology of the Central Nervous System.* Delivered at the Royal College of Surgeons of England, in May, 1858. By C. E. BROWN-SÉQUARD, M. D., F. R. S. Philadelphia: J. B. Lippincott & Co., 1860.

THE indefatigable and restless industry of Dr. Brown-Séquard has, from time to time, added many curious facts to our stock of knowledge, with regard to the functions of the nervous system. Beyond question, the most important of these acquisitions, which we owe to his genius and skill, are the discoveries which relate to the *transmission of the sensitive impressions*

[1] In the last number of this Journal is contained a very conclusive case of the kind, related by Dr. Clements, U. S. A.

in the spinal cord. These impressions were almost universally admitted, for a long period of years, to be transmitted by the fibrous texture of the posterior columns. The double function of the cord, as an organ of transmission, was thus thought to be precisely similar to that of the two roots of the spinal nerves. As the anterior roots of the spinal nerves were demonstrated to be the organs of transmission for the motor stimulus, and the posterior roots for the sensitive impressions, so the anterior columns of the cord were regarded as the exclusive channels by which the motor stimulus was conveyed downward from the brain, and the posterior columns as the only means of communication by which sensations found their way upward to the sensorium. It was natural, accordingly, to consider the spinal cord, at least so far as concerned the white substance of its anterior and posterior columns, as nothing more than a bundle of fibres continuous with those of the nerve-roots; giving off, from time to time, motor fibres to the anterior roots, and receiving, from time to time, sensitive fibres from the posterior roots.

An anatomical analogy was also found to exist between the anterior and posterior columns, in regard to the lateral decussation of their fibres. It had long been shown, beyond a doubt, that this decussation, for the anterior columns, must be located at the lower part of the medulla oblongata in front; the whole of the motor fibres of the right anterior column passing through the left side of the medulla oblongata, to the left side of the brain, and the whole of those belonging to the left anterior column passing through the right side of the medulla oblongata, to the right side of the brain. A similar decussation for the fibres of the posterior columns was pointed out, by various observers, at the upper part of the medulla oblongata, behind.

The physiological and pathological proofs of the locality of this decussation, however, were much less complete for the posterior columns than for the anterior. It was easily demonstrated that injury of one side of the brain produced paralysis, both of motion and of sensation, on the opposite side of the body; so that no doubt could exist of the complete decussation of both sets of fibres. But for the motor fibres of the anterior columns the precise spot of this decussation was indicated by the result of physiological experiment as well as by the researches of the anatomist. For while injury of the brain, as above mentioned, was always found to produce a paralysis of motion on the opposite side of the body, injury to the *spinal cord itself* on one side was followed by paralysis on the same side. Up to the level of the medulla oblongata, therefore, the motor fibres of the cord were shown to be in relation with the nerves and muscles of their own side of the body; above that level, with those of the opposite side. It seems to have been inferred a little too hastily that the arrangement of the sensitive fibres was a similar one, and that they all crossed over, from right to left and from left to right, at a single point, in the upper portion of the medulla oblongata, or just below the tubercula quadrigemina.

Dr. Brown-Séquard, however, has corrected this inference in a very important particular, by showing that the decussation of the sensitive fibres of the spinal cord takes place, not at a single point at or above the level of the medulla oblongata, but throughout the whole length, or nearly so, of its longitudinal extent.

The first experimental fact demonstrated by Dr. Brown-Séquard, in regard to this point, was a very unexpected one. He showed that if a complete sec-

tion were made, in the living animal, of one lateral half of the spinal cord, paralysis of motion resulted, as previously demonstrated, on the corresponding side of the body below the point of section; but so far from the power of sensation being lost in the same parts, the sensibility of the skin and subcutaneous tissues *was absolutely increased* on the corresponding side of the body. Further experiments indicated that the increase of sensibility under these circumstances was owing to an unnatural congestion of the parts following the section of the nervous fibres. But the most important fact thus ascertained was that section of one lateral half of the spinal cord does not destroy the sensibility of the parts below, on the corresponding side. The conducting fibres of the sensitive impressions do not, therefore, like those of the motor impulses, remain connected with the same lateral division of the spinal cord throughout their course.

This question was still further elucidated by Dr. Brown-Séquard, by an experiment consisting in the double section of the spinal cord at different points. He divided, for example, the right lateral half of the cord in the lower dorsal region, producing, as above, *hyperæsthesia* of the right posterior extremity; and afterward, on dividing the *left* lateral half of the cord, in the same animal, in the *cervical* region, he found that the right lower limb, previously hyperæsthetic, lost, in consequence of the latter section, all trace of sensibility in the integument and subcutaneous tissues. Thus both the negative and positive results of those experiments pointed to an entire decussation of the sensitive fibres in the spinal cord itself.

This result was rendered more striking by combining a section of both posterior columns of the cord with a complete section of one of its lateral halves. Both posterior columns, for example, having been divided, there is hyperæsthesia of all the parts behind the point of section. Subsequently the section of the entire lateral half on the right side being completed, the hyperæsthesia on the *right* side seems to be increased, while on the left side, all sensibility has disappeared.

By a combination of various other ingenious operations the experimenter perfected his demonstration still more thoroughly. Perhaps the most simple and at the same time most striking of these operations is that which consists in a *splitting*, or longitudinal division of the spinal cord upon the median line. The spinal cord is laid bare in the entire lumbar region, and throughout the whole of that portion which gives origin to the nerves of the posterior limbs, a careful section of the cord is performed, along the median line, so as not to injure the substance of either lateral half, but simply to divide the middle and commissural portion of the cord. This is, of course, an exceedingly delicate operation, and, as the author acknowledges, it is not possible to avoid entirely slight accidental injuries to one side or the other. It may be accomplished sufficiently well, however, as he says, to give very distinct and very important results. These results are that the *sensibility of both posterior extremities is entirely lost*, while their voluntary movements remain nearly unimpaired, thus demonstrating at the same time the decussation of the sensitive, and the non-decussation of the motor fibres in this part of the spinal cord.

This experiment, as Dr. Brown-Séquard states, was performed many years ago by Galen, who failed to notice, however, its curious effects upon the sensibility of the parts.

Dr. Brown-Séquard, however, is not content with showing the fact of the decussation of the sensitive fibres in the spinal cord. He endeavours

to establish the exact mode of this decussation and the entire route through which the sensitive impressions are conveyed upward to the brain.

In the course of his remarks on this question he takes occasion to discuss a very important point in regard to the proper mode of experimenting on the sensibility of the nervous system, and the conclusions to be drawn from certain results. This question is the following: *How are we to determine whether a particular part of the nervous system have or have not the power of transmitting sensitive impressions?* Dr. Brown-Séquard very properly remarks that certain experimenters have too readily taken it for granted that the true test for the existence of this power is the *sensibility or non-sensibility of the parts themselves.* They have supposed that a portion of the nervous apparatus which is itself sensitive must necessarily be the conductor of sensation from other parts; and, *vice versâ*, that a portion which is not sensitive cannot be a conductor of sensibility. They confound the two properties of transmission and sensibility, or regard them as necessarily coexistent.

Dr. Brown-Séquard, however, shows that there is no ground for such an assumption. The two properties mentioned above are quite distinct in their nature, and may exist separately or together, as the case may be. In point of fact, those parts of the nervous system which are sensitive, that is, in which painful or other sensations are excited by mechanical or galvanic irritation, are for the most part found to be also the conductors of sensibility; but the latter property is demonstrated by a different set of experiments from those which prove the existence of the former. We can ascertain whether a part be sensitive or not, by simply applying an appropriate cause of irritation, such as the galvanic current, and seeing whether a painful sensation be manifested in consequence. But in order to decide whether a part be the conductor of sensitive impressions from other organs, it is necessary to apply the irritation to these other organs, and to see whether its transmission to the sensorium be affected by the injury or division of the part under examination.

If this rule of investigation be applied, as Dr. Brown-Séquard contends, several parts of the nervous system, which are not themselves sensitive to external irritation, will be found to be undoubtedly the organs of transmission from other parts; since, if they be injured or destroyed, the parts below will be found to have lost their sensibility. This mode of experimenting becomes particularly valuable when it is claimed that such or such a part is the *exclusive* channel of transmission for nervous impressions. If the claim be a just one, the sensibility of the organs below must be *entirely* abolished by destruction of that part alone, the remaining connections being left untouched; and furthermore the transmission of sensations must remain perfect, provided that particular part be untouched notwithstanding all the other connections be entirely cut off.

Dr. Brown-Séquard does not believe that the posterior columns of the spinal cord are the exclusive channels for the transmission of sensitive impressions; and he thinks that this property has been too hastily attributed to them on account of their sensibility to external irritation—a sensibility similar to that possessed by the posterior roots of the spinal nerves, and which is not to be found in the anterior columns of the cord, nor in the anterior roots of the spinal nerves.

The author even seems to doubt the fact that the posterior columns of the cord possess so great an amount of sensibility as has been attributed to them. He thinks that this apparent sensibility has been exaggerated from

two causes: first, that a galvanic shock applied to the posterior columns must necessarily irritate at the same time the posterior roots of the corresponding nerves, so that the sensibility of the nerve-roots, which is very acute, has been mistaken for that of the columns, which is doubtful: and, secondly, that irritation of the posterior columns gives rise very readily to *reflex movements*, which have sometimes been mistaken for signs of pain. Even in the decapitated animal, as the author states, the reflex movements excited by irritating the posterior roots of the spinal nerves, at the same time with the posterior columns, are much more decided than when the irritation is applied to the posterior columns alone.

We do not think, however, that the author is quite right in attributing the usually received doctrine in regard to the posterior columns as organs of transmission, altogether to their supposed sensibility to direct irritation. This remark is particularly applicable to what is said of Longet on page 15.

"Mr. Longet," it is said, "thinks that the gray matter of the spinal cord cannot be a conductor of sensitive impressions, because it is not endowed with sensibility; and his single argument to prove that the posterior columns are the sole conductors of the sensitive impressions is that they are the only sensitive parts of the spinal cord."

Longet, in reality, adduces another, and, if we are to trust his results, extremely important observation bearing on this point. He says (*Traité de Physiologie*, vol. ii., part ii., p. 187):—

"Some physiologists having considered the gray substance of the cord as indispensable to the transmission, not only of sensitive impressions (Bellingeri), but also of the principal of the voluntary movements (Van Deen, Stilling, &c.), we would here say that, in the dog, we have constantly found the gray matter at the same time destitute of sensibility and incapable of exciting convulsive movements under the influence of electricity and mechanical irritations; *and that its destruction, to as great an extent as possible, by means of a stilet, has not been found to modify, in any way, either the sensibility of the posterior columns or the excitability of the anterior.*"

But whatever is to be thought of Longet's experiments on this point, and the value of his mode of operating, there is no doubt that Dr. Brown-Séquard has pointed out, in a very effective manner, the full requirements of the question at issue, and the danger of trusting too much to results obtained from direct irritation applied to the posterior columns themselves.

By his own experiments also he has shown very strong reasons for the belief that the posterior columns of the cord, though they receive the posterior roots of the spinal nerves, do not exclusively transmit their sensitive impressions to the sensorium above.

We have already alluded to the singular hyperæsthesia which he has found to result from section of the posterior columns.

"The first fact," he says (p. 19), "I have to speak of is, that a transversal section of the posterior columns, instead of being followed by the loss, or even a diminution of sensibility, seems to produce an increase in the amount of this property; in other words, I have found that the section of these pretended only channels of the sensitive impressions, instead of preventing them from passing, allows them, on the contrary, to pass more freely, so that instead of *anæsthesia* there is *hyperæsthesia*. In certain animals, and especially in rabbits and sheep, it is easy to ascertain that there is a very great increase in sensibility in the various parts behind the section. Before the operation, in rabbits, the most energetic pinching of the skin produces agitation, but no shrieking; after the operation, on the contrary, the least pinching produces shrieking and a much greater agitation. Sometimes the hyperæsthesia is so considerable that the

least pressure upon the skin makes the animal shriek. Whether the operation is performed in the lumbar, the dorsal, or the cervical region, the phenomena are always the same; that is, there is a manifest hyperæsthesia in the various parts of the body which receive their nerves from the part of the spinal cord which is behind the section. It has been so in all the animals I have operated upon, and I have already made this experiment upon animals belonging to more than twenty species.

As long as the animals live, after the section of the posterior columns, hyperæsthesia continues to exist, except in cases where reunion takes place between the two surfaces of the section; but hyperæsthesia is greater during the first week after the operation, than it is after a month or many months."

* * * * * * *

"If we carefully dissect the two restiform bodies, so as to separate them from the neighbouring parts, and if we divide them transversely at their two extremities and then remove them, we find that the animal, instead of losing its sensibility in the different parts of the limbs and trunk, becomes hyperæsthetic.

It results, from these experiments, that the restiform bodies, which are the direct continuations of the posterior columns of the spinal cord, are not the only channels for the transmission of sensitive impressions to the sensorium.

It seems certain, therefore, *that the posterior columns of the spinal cord and of the medulla oblongata are not the only channels for the transmission of the sensitive impressions from the limbs and trunk to the sensorium.*"

Dr. Brown-Séquard, however, arrives, by further experiments, to much more complete results than the foregoing; for he finds that after dividing by a transversal section, the whole of the spinal cord, *with the exception of the posterior columns*, sensibility is lost altogether in the parts situated behind the point of division. The sensitive impressions, therefore, in the natural condition of the parts, are *wholly* conveyed by some other route than that of the posterior columns. This route, the author believes, is an oblique one; the sensitive filaments, derived from the posterior nerve-roots, passing for a very short distance after their joining the spinal cord, by the corresponding posterior columns, and then entering the central gray matter of the cord, in which they afterward cross to the opposite side of the median line, thus giving rise to the decussation or crossed action of the spinal cord, in the transmission of sensitive impressions.

The difficulty in establishing this fact lies in the want of any sensibility as resident in the central gray matter. But this has already been shown to be no objection to the truth of the author's conclusions. The gray matter may have the power of transmitting nervous impressions from below, though it may not itself be sensitive to direct external irritation. It is not necessary to suppose that the same sensitive filaments, derived from the posterior nerve-roots, pass continuously through the gray matter of the cord to be afterward connected with the sensorium above. They may terminate in the substance of the gray matter, which is in its turn connected, by commissural longitudinal fibres, with the ganglia of the brain. This anatomical arrangement, in fact, has been heretofore suspected to exist, owing to the small size of the spinal cord at its upper portion, as compared with the immense number of filaments which it gives off below to the spinal nerves. It was thought that the cord could not, on account of this disparity in size, contain all the filaments derived from or given off to the anterior and posterior roots of the spinal nerves.

The author asserts the power of the central gray matter of the cord to transmit sensitive impressions, from the results of experiments quite different from those obtained by Longet, to which we have already alluded. Let us remember that division of the posterior columns of white matter alone

produces hyperæsthesia in the parts below, but no loss of sensibility; and that an increase of sensibility is also found to take place after division of the lateral columns. Now, Dr. Brown-Séquard finds that if a transversal section be made of the whole posterior half of the cord, in the dorsal region, so that, beside the posterior white columns and part of the lateral columns, a portion of the central gray matter is also divided, then there is a diminution of sensibility in the two posterior limbs. Since this diminution cannot have been caused by the division of the posterior or the lateral columns, it must have been owing to the partial section of the central gray matter.

Again, a similar section being made of the whole *anterior* half of the spinal cord, a similar partial loss of sensibility is found to take place in the posterior limbs, which cannot be the result of the injury to either the anterior or lateral white columns, and must therefore, as before, be attributed to the partial section of the central gray matter.

It will be seen that these experiments on the effects of section of the central gray matter are essentially the same with those performed by Longet, but yielding diametrically opposite results. Longet endeavoured to destroy the central gray matter of the spinal cord by the use of a stilet introduced longitudinally, leaving the white columns untouched; while Brown-Séquard divides successively the anterior and posterior halves of the gray matter, together with their corresponding columns of white substance, having previously ascertained that division of the white columns alone will not produce a loss of sensibility. We do not undertake to decide to what cause the discrepancy between the two is to be attributed.

The oblique course of the sensitive fibres, after their entering the spinal cord from the posterior roots of the nerves, is shown by successive sections of the posterior columns and of the gray matter. The author finds that, if the posterior columns of the cord be divided a very short distance above the point where they are joined by the posterior roots, these roots lose their sensibility—showing that, for this distance, it is transmitted by the posterior columns. If the section of the columns be made a little higher up, the sensibility of the posterior roots is diminished, but not destroyed—showing that at that point some of the sensitive fibres have left the posterior column to reach another part of the cord; and if higher still, the sensibility of the posterior roots remains unaffected, because all their filaments have now left the posterior columns, after passing for a time obliquely through their substance. But if the central gray matter of the cord be divided at this last point, according to Dr. Brown-Séquard, the sensibility of the posterior roots immediately disappears, since their organ of transmission has been destroyed.

It is by a combination of many ingenious experiments similar to those which we have quoted above, that the author fortifies his position, and makes it, at least, in the highest degree probable that the transmission of sensitive impressions in the spinal cord takes place by a continuous and oblique decussation, and through the substance of the central gray matter.

The first four lectures are occupied with the discussion of the above questions. Lectures 5th to 8th inclusive contain various conclusions of a similar nature deduced from pathological cases, which the author studies and records in close connection with his experimental observations. Among these are cases of alteration of the whole spinal cord with preservation of sensibility; cases of alteration of the gray matter alone, with loss of sensibility and voluntary movements; cases proving that the decussation of the

channels of sensibility takes place in the spinal cord, and not in the medulla oblongata; cases of loss of voluntary movements in one side of the body, and of sensibility in the opposite side; paralysis due to disease of the gray matter; anæsthesia without paralysis, &c. &c.

The author recognizes very fully the complete distinction between the different kinds of sensibility—as, for example, sensibility to pain, to tactile impressions, to the effect of tickling, and to variations in temperature. It has been now for a long time known that these sensations are not the same in kind, but must be regarded as dependent upon quite distinct impressions upon the nervous system; for the sensibility to pain may be quite acute when that of ordinary touch is dull, and *vice versâ*. In fact, a painful impression, whenever it exists, actually interferes with the healthy appreciation of the qualities of foreign bodies by the sense of touch; and when the contact of a foreign body, by the suddenness or violence of its application or by the intensity of heat or cold, becomes painful, we can no longer perceive with any distinctness its true physical properties, but only the suffering which it produces.

Dr. Brown-Séquard has been led to believe that these different kinds of sensibility are not only distinct in their nature, but are actually *transmitted to the sensorium by different channels in the nervous system*. He does not, in the present work, give all the reasons which have led him to adopt this view, but he gives in the eighth chapter an analysis of certain cases of alteration in the spinal cord which go to show the probability of there being in this organ a special place of passage for some of these peculiar impressions. He believes that they all pass, however, by different channels through the gray matter of the cord.

These are: Cases of the loss of tactile sensibility, of the power of feeling pinching and pricking, and of the muscular sense, with persistence of the power of feeling cold and tickling. Loss of the power of feeling tickling or the simple contact of foreign bodies, with persistence of the sensibility to pain. Loss of tactile sensibility, with increased sensibility to painful impressions. Loss of the power of feeling heat, while tactile sensibility remains.

The ninth chapter is occupied with the sympathetic nerve and the effects of its division and galvanization on the circulation and state of sensibility in the parts beyond. It is well known that Dr. Brown-Séquard attributes all the results of this nature following division or irritation of the sympathetic nerve, to a temporary paralysis or contraction of the bloodvessels, and consequent change in the supply of blood circulating in the capillaries. This chapter, however, is more particularly of interest here, as being introductory to the succeeding one, in which the author speaks at length of the different kinds of reflex action as influencing, respectively, muscular contraction, the flow of the secretions, and the nutrition of the tissues. In each of these cases the reflex influence may be either positive or negative in its results—may produce either an increase or a diminution in the activity of the part. Thus we may have, in the case of the muscular system, either a reflex convulsion or a reflex paralysis. Reflex paralysis, in fact, is a subject which the author has made peculiarly his own, not only by bringing out distinctly the fact of its existence, but also by pointing out its different varieties, and by indicating the leading peculiarities of its treatment, as distinguished from that of ordinary or *direct* paralysis. In the same connection he speaks of the sudden arrest of the heart's movements, which sometimes takes place by a reflex action—the cause of

rapid death after injuries of the abdominal sympathetic nerve—stoppage of the heart's movements by the application of cold to the skin, by the influence of cold drinks, and in some cases of death by chloroform—muscular atrophy due to an irritation of sensitive nerves, &c. &c. This entire subject of the various reflex influences at work both in the healthy and pathological conditions of the system forms an exceedingly varied and interesting topic for investigation. It has grown wonderfully since the first experiments were performed upon the reflex convulsive movements in the voluntary muscles due to irritation of the skin in a decapitated frog. It has been shown, in fact, to enter more or less fully into nearly all the functions of the living body, those of growth and nutrition as well as those of sensation and muscular activity. No one, it may fairly be said, has followed out this subject so fully, or pursued it so successfully into its varied ramifications, as the author of the present work.

The eleventh chapter is devoted to a subject closely connected with the preceding and naturally following upon it, but which is still more peculiarly the scientific property of Dr. Brown-Séquard, *i. e.*, the *artificial production of epilepsy in the lower animals.* One of the most curious results ever attained by experimenting upon the nervous system was that which excited the attention of the profession some years ago, when our author brought out his unexpected discovery that a convulsive affection, closely resembling if not identical with epilepsy, might be produced in the guinea-pig, in consequence of a mechanical injury to the spinal cord, usually in the dorsal or lumbar region. A few weeks after the injury of the spinal cord, and when the immediate effects of the operation have much diminished or have passed away altogether, epileptiform convulsions appear, and are repeated daily, or once in two or three days. But the most important peculiarity of the convulsions, in this connection, is that they can be excited at will by pinching or otherwise irritating a particular point of the skin, situated over the angle of the lower jaw. No other part of the integument, according to the author, is capable of exciting these convulsions on being irritated, and the irritation of this point is almost invariably followed by a fit, so long as the affection continues. Dr. Brown-Séquard has frequently demonstrated these curious facts, since their discovery, to the students and medical men who have attended his various courses.

The epilepsy in these animals is, therefore, a *reflex* convulsion; and the author, in applying the results of the experiment to pathological conditions, treats of epilepsy in general as a reflex affection, depending, very probably, in the first instance, on some injury or disease of the spinal cord, and afterwards excited, so far as regards each particular attack, by an impression produced upon the integument or some part of the mucous membranes. It is unnecessary to point out the extremely interesting nature of these hints on the nature and mode of access of epileptiform attacks, or the valuable suggestions as to treatment which may be derived from them. They are very fully followed out, in this and the following chapters, and will abundantly repay perusal, both in a practical and scientific point of view.

We are inclined to believe that the present volume will be found to be the most valuable and successful which has yet emanated from the fertile pen of Dr. Brown-Séquard. J. C. D.

ART. XIII.—*Rélation Médico-Chirurgicale de la Campagne d'Orient, du 31 Mars, 1854, occupation de Gallipoli, au 6 Juillet, 1856, evacuation de la Crimée.* Par le Dr. G. SCRIVE, Médecin-inspecteur de service de santé des armées, ex-médecin en chef de l'armée d'Orient, &c. &c. Paris, 1857. 8vo. pp. 485.

A Medico-Chirurgical Account of the Crimean War, from the first arrival of the troops at Gallipoli, to their departure from the Crimea. By Dr. SCRIVE, Surgeon-General of the Army in the Crimea.

THE study of the medical and surgical history of the English and French armies engaged in the Crimean war is eminently instructive. The collection of a very large number of troops in a small territory, and the stability imposed upon them by the necessities of the principal object of the enterprise—the taking of Sebastopol—while they were at the same time actively engaged and living as soldiers in a campaign, afforded a most complete opportunity of observing the development of the diseases of armies, with their special etiology and pathology. The presence side by side of the two independent armies, which, though engaged in the same enterprise, were completely separate in their organization, and at times very differently occupied, gave, moreover, an unusual opportunity of judging what part climate and season had in causing these special affections, and what part must be assigned to the abnormal conditions inherent to the life of a soldier. In surgery, also, the experience afforded by the campaign was most extensive. The number of serious gunshot wounds that occurred was enormous. The world never before witnessed such a display of physical and material force exerted for so long a time, for purposes of destruction.

The volume of Dr. Scrive gives a very complete medical and surgical history of the French troops engaged in the Crimean war during its whole period. From this history we design to take those facts and reflections that appear to us most interesting and most useful; when appropriate, we shall connect with them those given elsewhere by English observers, regarding the English troops; and we shall do this with reference to the actual condition of affairs in this country.

The Crimean war lasted, without any intermission, winter and summer, for twenty-four months. The total number of French troops sent to the East, at different times, amounted to 309,268 men, of whom 200,000 entered the ambulances and hospitals to receive medical aid; 50,000 for wounds, and 150,000 for diseases of various kinds. The total mortality was 69,229, or $22\frac{1}{3}$ per centum. Of these, 16,320 died of wounds, and nearly 53,000 from diseases. It is of these diseases we wish to speak first, and we shall see at once what part climate had in their causation and their fatality, and what part the peculiar conditions of a soldier's life. The following table, taken from page 345 of Dr. Scrive's book, giving the statistics of the ambulances of the Crimea, will greatly aid in the comprehension of this question:—

	Entered.	Discharged.	Sent to Constantinople.	Died.
Officers wounded, ordinary wounds .	135	104	31	None.
Officers with gunshot wounds . . .	1,625	740	770	115
Officers with fever	1,098	401	503	194[1]
Soldiers wounded, ordinary wounds .	5,582	3,168	2,154	260[2]
Frostbites	5,596	2,012	3,472	112
Soldiers with gunshot wounds . . .	35,912	10,178	22,121	3,613[3]
Intermittent fever	6,983	3,746	3,197	40[4]
Remittent fever	12,267	4,036	6,436	1,795[5]
Pernicious fever	275	73	52	150[6]
Typhoid fever	6,351	1,060	1,628	3,663[7]
Typhus	11,124	1,266	3,840	6,018
Diarrhœa	19,339	5,240	12,115	1,984[8]
Dysentery	6,105	1,252	2,792	2,061[9]
Cholera	12,258	3,049	3,196	6,013
Scurvy	23,365	4,550	17,576	639[10]
Feverish	42,453	6,902	34,420	1,731
Venereal	1,455	1,201	241	13[11]
Itch	1,255	1,128	124	3[12]
	193,178	50,106	114,668	28,404

If we follow those sent from the ambulances of the Crimea to the hospitals of Constantinople, we have (at page 485) the following statistics:—

	From Varna and the Crimea.	By ticket.	Went out.	Sent to hospitals in Turkey or in France.	Died at Constantinople.
Wounded, ordinary wounds	2,185	1,007	2,059	720	413
Wounded by gunshot . .	22,891	None	9,619	8,190	5,085
Frostbitten	3,472	142	2,009	775	830
Typhus	3,840	4,889	3,544	1,778	3,407
Cholera	3,196	2,570	2,529	1,076	2,161
Scurvy	17,576	3,851	9,587	8,460	3,380
Feverish	63,124	8,038	35,625	22,988	12,549
Venereal	241	2,597	2,316	522	None
Itch	124	156	256	24	None
	116,649	23,250	67,541	44,533	27,825

Nearly 5,000 men died at Varna, at Gallipoli, and other places, before the troops reached the Crimea, a very large majority of them from the fearful outburst of the cholera in the Dobruscha. This number, added to 7,500, the number of those who were killed in engagements, or disappeared, together with those who died in the ambulances and hospitals, as stated above (56,229), gives the whole number lost by the French army.

Inspection of these tables shows that a very large proportion of the whole mortality is attributed to cholera, to typhus, and to scurvy, and great as it is, the true proportion of deaths directly caused by their presence in

[1] Many died of typhus and of cholera. [2] Complicated with typhus.
[3] Complicated with cholera, typhus, and scurvy.
[4] From complications. [5] Often complicated with typhus.
[6] Complicated with cholera and typhus. [7] Many of these cases were typhus.
[8] Complicated with cholera and scurvy. [9] Idem.
[10] Complicated with typhus. [11] From complications. [12] Idem.

the army is very much greater, for all other diseases as also the wounds, to a very considerable extent owed their fatality to complication with one or more of these three affections. In the whole campaign Dr. Scrive says that 11,000 men died of cholera out of 18,400 cases (p. 406). The total number of deaths owing to typhus he computes at nearly 18,000 out of 35,000 cases (p. 421). The loss from scurvy, according to the tables given above, is but little over 4,000; yet while it only exceptionally caused death of itself, the consequences of its invasion upon the troops were most disastrous, by preparing the ground for other affections which, associated with scurvy, gave rise to a very large proportion of the mortality (p. 428). Another consequence of the presence of the scorbutic influence was the loss to the army, for several months, the time required for its treatment, of the services of a very large number of the oldest and best trained soldiers, who were those principally attacked. As is seen above, over 23,000 cases of scurvy entered the ambulances of the Crimea.

Now of these diseases typhus and scurvy are undoubtedly *preventable;* that is to say, they are the effect of the action of causes that are known and that are under our control. Climate and season may interfere with our power of acting upon the causes of these diseases, but that is as far as their influence extends in the matter of their production. With proper food, good clothing, pure air, cleanliness, and sufficient rest, neither scurvy nor typhus fever can arise.

In cholera, the effect of the influence of the peculiarities and hardships of the soldier's life was seen, not in producing the disease as in scurvy and in typhus, but in enormously increasing its mortality. Cholera acquires an extraordinary power of destruction by association with ordinary great disturbing and disorganizing morbid causes, as ill-ventilated habitations, the concentration of animal and vegetable miasmata, overcrowding, great fatigue, excited passions, and painful privations—in other words, with the conditions almost necessarily attending the life of the soldier in active service.

It was while making forced marches, in the end of July, through the Dobruscha, a country without any resources, and infected with marsh miasma, that the cholera made its appearance among the French.[1] By

[1] We cannot here go into any particulars in regard to this fearful epidemic. To give an idea of its violence, we may state that in one day, in a single regiment, 300 men were attacked, almost all fatally. We will be excused, however, for copying from Dr. Scrive's work the following picture of the landing of the suffering troops, on their return from the Dobruscha, at Varna, where they were carried on steamers: "I never assisted at a more frightful spectacle than the one we witnessed on the beach at Varna, when these poor soldiers were landed, rendered unrecognizable by the terrible pestilence that had attacked them. Once in particular, it was evening, and the uncertain light of the moon added still more mournful shades to the picture. The sick were hoisted from the boats by sailors and deposited on the sand of the beach; some, completely collapsed, allowed themselves to fall heavily; others having preserved a remnant of strength, walked a few steps like drunken men, or crawled along on their hands, and soon fell motionless in their turn, from complete exhaustion of their strength. Some were naked, or almost naked, or only with pieces of dress thrown over them that did not belong to them. Officers and private soldiers lay together pellmell on the sand; all ranks were confounded in the presence of death. Many of them begged for drink in that broken, sepulchral voice peculiar to the disease; others cried out or groaned from the excruciating pain of the cramps. Those who died while being carried ashore were placed in a line on the bank, each one in the position in which the agony of death had left him." (pp. 79, 80.) What a picture this would be for the pencil of the great artist who represented the cholera on board of the Melpomène!

practising the three great principles of complete isolation, constant aeration, and permanent dissemination of the men affected, its ravages were checked at once. Three months afterwards, in the Crimea, after the battle of the Alma, the army was forced, from want of provisions, to remain on the field, which was covered with dead horses, putrefying and giving out an insupportable odour. The cholera burst out again. The climate of the Crimea was far from being favourable to the spread of cholera; on the contrary, again and again, when carried there by the arrival of troops from France and Turkey, it disappeared in a few days from the lack of conditions favourable to its development. (Scrive, page 86.)

In fact, with the exception of the intermittent fevers caused by encamping for a period of time in the neighbourhood of the marshes of the Tchernaïa, fevers which when uncomplicated yielded promptly to simple treatment, there was nothing in the diseases of the French army that could be laid to the account of the climate of the Crimea, which is analogous to that of Corsica, of Italy, and of the south of France.[1] The extreme seasons, winter and summer, could have had no particularly bad influence upon the health of the troops, had they not been undergoing the fatigues, privations, and hardships of war. It can safely be concluded, therefore, both from the nature of the diseases by which the French soldiers were destroyed, and also from the universally admitted salubrity of the climate of the Crimea, that the great mortality among them was dependent upon causes inherent to the life of a soldier, in an active campaign, by which are comprised life in camp, the food furnished to an army, the clothing, the habitations, the concentration of large masses of men, and the fatigues, hardships, and moral effects of a war.

Very strong additional proof of the predominating influence of these particular causes in the production of disease during the Crimean war, is to be deduced from a consideration of the relative conditions of the English army at the beginning and at the end of the campaign. The English sent to the Crimea 93,959 men. Of these there were killed in action and died of wounds 4,446; the deaths from disease were 16,298; and 12,903 were invalided. The whole mortality was therefore over 22 per cent., or nearly the same as that in the French army. Now at least five-sixths of this mortality occurred during the first six months, from typhus, scurvy, frost-bite, dysentery, and diarrhœa, which had one common origin, namely, the exhaustion of the vital powers from overwork, inadequate night-rest, unsuitable clothing, inappropriate shelter against wet and cold, scarcity of fuel for cooking, unwholesome food, and insufficient nutriment. At the same time that the soldiers were dying so rapidly, the officers, exposed to the same weather and influences of climate, were more healthy in the Crimea than in England. During the second winter the health of the men was so perfect, that in the month of January, the deaths were only 124 in all the hospitals, regimental and general, in the Crimea and on the Bosphorus. The report for the week ending the 15th March, gives 70,409 as the total number of troops, of whom only 3,747 were under medical treatment, and the fatal cases on these seven days numbered only 19. At this time the influences of climate were the same, but the whole army was well clothed,

[1] To prevent as much as possible the action of the miasma from the marshes, the orders of Dr. Scrive were to keep up fires around the camp at night, to commence work late in the morning, to supply food and tonic drinks before proceeding to work, to wear flannel, to hermetically close the tents at night, to provide good food, and to allow the men to rest frequently while engaged in hard labour.

well sheltered, and fed; while the military duties of the men served only for healthy bodily exercise.

These facts in regard to the condition of the English forces we have taken from the several articles published in the *British and Foreign Medico-Chirurgical Review*, at the time of the Crimean war, or soon after its termination. In one of these articles, and a very ably written one it is, styled "The Allied Armies before Sebastopol,"[1] the writer discusses the question of the relative merits and demerits of British and French military administrative regulations in preserving and restoring the efficient health of soldiers in the field, and the conclusion to which he comes is that the extraordinary sanitary condition of the British troops during the last winter in the Crimea, while the French army was reduced three-fourths in numbers and efficiency by camp sickness, hospital destitution, and the failure of its medical administrative resources, is mainly due to the great superiority of the British military organization. Such a conclusion is, to say the least, a very unwarrantable one; but it was published when the war was over, and could produce no injurious effect, while it would be gratifying to see it proclaimed. In the beginning of the war, in the same Review (in the number for April, 1855) we read as follows:—

"The deeply humiliating spectacle has been exhibited of a British army, unequalled in daring and discipline, perishing of want in the vicinity of abundance; naked and tattered within sight of stores of clothing; clothed, fed, and transplanted when sick, by a gallant and generous ally, to whose superior organization it owes its very existence."

Most unquestionably the true reason for the superior health of the English troops during the second winter is given by the reviewer himself a few pages preceding, where he says—as we have repeated above—that the whole army at that time "was well clothed, well sheltered, and fed; while the military duties of the men served only for healthy bodily exercise."

In fact, after the destruction of Sebastopol, in September, the English did no more than make themselves comfortable, while protected by the marches and counter-marches of the French. While the latter, a gun in one hand and a pickaxe in the other, were fortifying and defending a line of more than twelve leagues from Sebastopol to Baidar, three-fourths of the men on the watch all the time from constant alarms, obliged to go far to seek their wood, water, and provisions, the thermometer sometimes down to zero, and once even eleven degrees below (—24° Centigrade), with only *tentes creusées* and *huttes en torchis* for their habitation, the English were most comfortably installed in well constructed wooden barracks, thoroughly warmed and ventilated, well paved roads, and even railroads, bringing an abundance of fuel and provisions into the camp, and so many labourers from Turkey, Greece, and England, to do all the hard work of the camp that they equalled in number one-half of the army.

It does not accord with our object in writing this article to discuss at all the relative merits of the organizations of the French and English armies. It is, we are firmly convinced, a fact that cannot fairly be questioned that the greater mortality of the French army during the last winter was owing to the conditions we have just given, or in other words, to their being more exposed, and to an extraordinary degree, to the deleterious causes of disease inherent to a soldier's life, and we believe, moreover, this great mortality fell upon them in spite of their organization, of the cha-

[1] In the number for January, 1858.

racter of which we have seen the English writer bear witness, since he states its superiority to have saved the perishing British army.

The French military surgeons are eminently qualified for contending against the diseases peculiar to armies. They pass through a long course of study in special schools and hospitals, and they have had ample practical experience in their constant wars in Algeria and elsewhere. Their scientific merits, ardent zeal, self-denial, and absolute devotion, as manifested throughout the whole Crimean war, are beyond all praise. No suffering soldier ever wanted a dressing, a remedy, or a consolation. Of 550 that were necessary during the campaign, 83 were killed or died from disease. Dr. Scrive says, moreover, that there was not one single judicious measure advised by the surgeons that was not at once ordered to be executed by the chief of the administration.

If, therefore, during this war, notwithstanding all the skill and zeal of the medical men, eagerly seconded by the administration, the mortality from diseases was enormous, and these diseases were nevertheless mainly of a kind known as *preventable*, we see how very difficult it may become in an active campaign to overcome their causes, preventable though they be, and how readily armies are affected by them. These causes, as already said, are the vitiation of the air from over-crowding and from want of strict attention to cleanliness, improper food, imperfect protection from habitations, insufficient clothing, and over-fatigue. We will examine as briefly as possible the measures enforced by Dr. Scrive for counteracting these causes, and wherein and why these measures failed.

The greatest care was always taken by the surgeons that there should be a sufficient space between the tents to allow of free circulation of air; and that the kitchens, the stables, and the privies were removed to a sufficient distance. Every morning early, if the weather permitted, the clothing and bed coverings of the soldiers were exposed outside the tents, which were left widely opened, in order to allow the complete aeration of the ground, and of their being swept. In some places twice a week the tents were taken down in order to expose the ground most fully to the sun and air; in other places, this was not done so often. The rubbish of all kinds was every day taken to a distance from the camp and burned. Spots of infection in the neighbourhood of the camp were destroyed when possible by burning; if not possible, the disinfecting properties of sulphate of iron were resorted to. The sulphate was dissolved in water, in the proportion of one to fifteen, by weight, and three quarts were used to every square yard of ground. The burials were made three feet deep; if the nature of the ground was such as to make this impossible, the bodies were covered with quick-lime. The products of the slaughter-houses and of the sewers were treated in the same way. The tents or barracks were disinfected by using a solution of chloride of lime, one part of the chloride to twelve of water, stirred, and left to settle; one wineglassful of the clear liquid was placed in the tent, and this was done in the morning, so that the exhalation of chlorine might not annoy the men while sleeping. The tubs serving as privies had, every day after they were cleaned, a quart of the solution of the sulphate of iron thrown into them. Men attacked with infectious or contagious diseases were completely isolated from the others, disseminated here and there, and provided abundantly with fresh air. When lightly attacked with typhus, six or eight cases were placed in a tent for sixteen men; when seriously attacked, only four. In cold weather a coal

fire was kept brightly burning in the centre of the hospital tents or barracks for purposes of warmth and ventilation.

The food given to each soldier was, for every day—

Bread, 750 grammes (1½ lbs.), or biscuit, 550 gr. (nearly 1⅛ lb.). Meat, 300 grammes (nearly 10 oz.), or salted pork (a little over ½ lb.). Rice, 60 gr. (nearly 2 oz.). Sugar, 20 gr. (a little over ⅝ oz.). Coffee, 16 gr. (a little over ½ oz.). Salt, 16 gr. (a little over ½ oz.).

Fresh meat was given four days in ten; salt pork, three days; and three days, preserved meat. The sick had fresh meat every day. An extra ration of wine was given every day, and also an extra ration of 100 grammes (a little over 3 oz.) of biscuit. Three times a week a quarter of a litre (or more than half a pint) of wine, or in its place one-sixteenth (a little over f℥ij) of brandy four times a week. Rice, beans, sugar, coffee, potatoes, and onions were sometimes given as substitutes to other things.[1] The dandelion, which grows very abundantly in the Crimea, was eaten as salad whenever it could be procured. When there was none to be had, in the severe cold of winter and the long-continued droughts of summer, its influence in preventing scurvy was manifest to a most surprising degree.

The whole army during the second winter, when the sickness was greatest, was in under-ground huts or under large tents; the small-sized ordinary tent was no longer used.

As to their covering, every man had a criméenne (a sort of hood), gloves, socks, long stockings of wool, Bulgarian gaiters, chéchias, wooden shoes, and overcoats of sheepskin for mounting guard and for any extra duty.

During the second winter, when the sickness was greatest, the French army was compelled to defend a line of more than twelve leagues. At every moment there were alarms keeping under arms three-fourths of the men. The enormous extent of the line of defence, requiring daily numerous *avant-gardes* and *grand' gardes*, rendered the provisioning of the army both difficult and fatiguing, and spread it out over all kinds of ground favourable or not to a winter encampment. In other words, the French troops were then subjected to excessive fatigue, in the most inclement season of the year, and in camping-grounds often wet and unhealthy; with habitations quite insufficient to protect them from intense cold, the humidity entering by the floor, the sides, and the roof, and containing too many men, thus reuniting the most favourable conditions to the production of cachectic and infectious diseases.

According to Dr. Scrive, the food of the French soldier, which, as is seen, does not differ materially from that of our own, should be reformed. When the live stock following the army became diseased, as was the case when it was attacked both by cholera and by typhus when these epidemics prevailed among the troops, or could not be transported to the seat of war, as was the case when the navigation in the Black Sea was prevented by violent storms, salted meat was all they had. Salted food, Dr. Scrive says, should be banished as much as possible from the provisioning of armies. The great progress of alimentary chemistry furnishes, at the present day,

[1] The daily ration of the American soldier, which we place here for facility of comparison, is: Pork ¾ lb., or 1¼ lb. of fresh or salt beef. Flour 18 oz., or hard bread 12 oz., or 1¼ lb. of corn-meal. And for 100 rations he receives 8 quarts of peas or beans, or 10 lbs. of rice, 6 lbs. of coffee, 12 lbs. of sugar, 4 qts. of vinegar, and 12 qts. of salt. In campaigns, the quantity of hard bread is increased to 1 lb. Fresh beef is required to be furnished as often as the commanding officer may require, or at least twice a week.

precious resources from which great profit might be drawn for the health of soldiers. If fresh meat cannot be had, it should be replaced by preserved juice of meat, concentrated soups, and boiled beef. These, he says, rendered accidentally immense service at the end of the Crimean war. He advises, moreover, that fresh vegetables, reduced by desiccation to a very small size, and which, with proper care, can be thus indefinitely preserved, should be regularly served in the rations of soldiers in active service, in equal parts with the rice and peas or beans. In this way a variety of food absolutely requisite to the human organization would be furnished, and the soldiers would be infallibly preserved from the scurvy. In modern times no case of scurvy need ever occur at any season of the year in an army, provided, of course, its communications are kept open.

Prophylactic measures against typhus are far more difficult of execution in an army, and sometimes, as in the Crimea during the second winter, they are impossible. Nevertheless, every possible sacrifice must be made to prevent its being engendered. Excessive fatigue must be avoided, the food must be good and the water drinkable, rules of general and individual cleanliness must be imposed, and the men must be placed where they will breathe a pure and renewed air. When it does break out, those attacked must be separated from the rest and scattered here and there in the most healthy spots that are accessible. In the Crimea, during the last winter, human means could do nothing; the weather prevented hygienic measures almost entirely, and there was no possibility of transporting the sick to other places, on account of the storms in the Black Sea. As to the extraordinary fatigues and hardships undergone by the French soldiers, these were rendered absolutely necessary by the exigencies of the war.

Together with the large armies sent by England and France to the Crimea, was a force of 12 or 15,000 men furnished by Sardinia. Their military organization is almost exactly the same with the French, and we see nothing additional to what we have already stated to be learned from their sanitary condition, except the great advantage they had, according to Dr. Scrive, from giving the entire direction of the hospitals, as regards the arrangements of the sick, the furniture, the kitchen, the pharmacy, &c., to the Sisters of Charity, who, he adds, can never be replaced by men, who have not the same feelings as women, above all, women animated by the sentiments actuating the Sisters of Charity.[1] (p. 302.)

Having now become acquainted, from the study of the medical history of the Crimean war, with the diseases from which an army in the field is liable to suffer, and with the extent to which it may suffer under unfavourable circumstances; and also with the causes of these diseases and the means to be adopted for their prevention, it remains for us to make what use we can of the information thus derived, and to endeavour to profit by it.

It is to be remarked, that all those difficulties to the removal of the causes of these diseases, dependent upon the great distance of the seat of war from the mother country, do not exist in this country, under present circumstances. It is only from ignorance, from want of zeal in the discharge of duty, or from fraud, that our men can be led to suffer in provisions, clothing, medical care, or hospital attendance. Well fed, well clothed, and protected from the weather, and properly cared for by the medical officers, no amount of

[1] While cordially agreeing with what Dr. Scrive says of Sisters of Charity, we think it necessary, in order to avoid misunderstanding, to declare that there are serious objections to the employment of female nurses for soldiers. The experience of military surgeons has fully settled this question in the minds of medical men.

fatigue they will be called upon to undergo, can ever be so long continued as to exhaust their vital powers and lead to a great epidemic. Upon those employed in furnishing, receiving, and distributing the necessary supplies of every kind for the army, and upon the medical officers attached to it, will depend, therefore, the health of our troops. As to the soldiers themselves, it is well known as a rule all the world over that they abandon themselves recklessly to the various chances of their fortune, good or bad, live from day to day, and neglect every precaution for preserving health. They require everywhere a superior direction, regulating by discipline all the physical and moral acts of their life. As to our volunteers, in the war with Mexico, we see that in a ten months' campaign the total loss was 21.31 per cent., or 2.13 per cent. a month, and their loss from disease relatively to that of the regulars was as 4.7 is to 1.

The necessity of scrupulous care in the selection of the medical men attached to our army is therefore most evident. In the way in which diplomas are given in this country, men become members of the medical profession who are altogether unequal to the duties they may be called upon to perform, even in ordinary circumstances. In civil practice this is not of very great importance, for the beneficial effects of competition correct the mischief. The public is at liberty to choose, and it is in a great measure their own fault if they suffer from falling into incompetent hands. The soldier, however, has no option in the matter, and he must submit to whatever the government may give him. A quantity of gilt-cording attached to their clothes, and the possession of two or three little books professing to treat of military surgery, though they are so small as to be readily carried in the pocket for easy reference, cannot render all the members of our profession fit for the duties they are to be called upon to perform. Before receiving an appointment in the army, they should be carefully examined by competent men. Moreover, we feel called upon to declare, that to preserve the health of the troops, it will be absolutely necessary to place every thing relating to their hygiene in the hands of members of the medical profession who have devoted their lives to such studies. Political demagogues, fanatical ministers, and strong-minded women, should be sternly repelled from intruding here. Too many lives, too great interests are now at stake.

Though our main object in writing this article is to call attention to the diseases by which armies are disabled, and to the means of preventing them, we will, before closing, make some brief extracts from what Dr. Scrive says on the subject of military surgery, or upon the care to be given to the wounded in engagements, and the treatment of gunshot wounds.

When the troops were engaged in besieging Sebastopol, as the wounded were brought in from the trenches they were directed by the administrative officer on guard into the tents *of waiting*, attached to the ambulances.[1] They were deposited in these tents in the order of their position as they became gradually filled, and in each tent they were placed in a regular order from right to left beginning at the entrance. One or more surgeons visited rapidly the wounded thus placed, and those whose wounds appeared to demand a severe operation were transported at once to two tents spe-

[1] An ambulance in the French service means an establishment for the temporary succour of the sick and wounded. It follows the movements of the troops for the purpose of affording the first attentions necessary. Each ambulance has at its disposal eighteen complete tents, a large quantity of bed furniture, and six hundred dressings for the wounded. Thirty-four men are attached to it, surgeons, apothecaries, administrative officers, and male nurses.

cially designed for the purpose. Simple dressings, or small operations to be quickly performed, were then executed, following the order in which the wounded had entered. The amputations and capital operations were performed afterwards when the crowding had ceased and the time they required for their careful performance could be more conveniently bestowed upon them.

At the taking of the Malakoff, when, as was anticipated, the number of wounded was very great, the medical service in the ambulances of the trenches was thus constituted: on the left were ten surgeons, and in addition an *ambulance volante* to follow the assaulting columns; on the right, twelve surgeons together with an *ambulance volante* composed of four surgeons; at Karabelnaïa, ten surgeons and a third *ambulance volante*. As the wounded were brought in, they were placed regularly in groups; each group was dressed by a surgeon having under his orders two assistant surgeons, and three male nurses, one to carry apparatus, the second to assist the surgeons, and the third to write on a paper prepared beforehand the information given by the wounded man as to his name, his first names, his regiment, and his number, and also whatever the surgeon might indicate as to the nature of the wound. The wounded, so soon as dressed, were sent on litters or *cacolets* to the division ambulances. Those upon whom serious operations were thought necessary were carried to one place where certain surgeons especially appointed to this purpose performed these operations. The assault commenced on the 8th September, at half past twelve in the afternoon. At Karabelnaïa 900 wounded, and on the right 1,800 were thus attended to in less than eight hours; on the left, where the fighting began some two hours later, 660 were attended to in six hours, and by eight o'clock in the evening the division ambulances had each received its allotted share. On the right, where the struggle had been hardest, the wounded continued to be carried in during the whole night, and even on the following day. The whole number of wounded thus cared for at the capture of the Malakoff was 5,000, of whom 400 were Russians. By the 10th of September all the wounded had been dressed for the second time, and of 550 capital operations to be performed, 350 were already done.[1]

Among the gunshot wounds received in the Crimea were many of a new kind, produced by cylindro-conical balls, about which there was but little experience before that campaign.[2] The extreme swiftness of these balls makes them go through any organic tissue, however dense and resistant, without deviating from their course. In cases where soldiers were struck, creeping on the ground, these balls went straight from one end of a limb to the other, through bone and all, without changing their direction. Macleod, in his *Notes on the Surgery of the War in the Crimea*, says that one of these balls never comes to mark a bone with any touch more gentle than what occasions its utter destruction. The consequence of these wounds, according to Dr. Scrive, is a traumatic inflammation extremely violent, extending over a vast surface, leading frequently to strangulation and gan-

[1] The number of medical men upon whom this heavy labour fell was only 80, and they were compelled at the same time to attend to more than 5,000 other patients, sick and wounded, already in the ambulances. There was but one medical man to every 130 patients, and there should always be two to every 100.

[2] The rifle used by the Russians, according to Macleod, was little inferior in range or force to the Minié, while its conical deep-cupped ball was much heavier. Their range was 1,200 yards, and the weight of the ball 1 oz. 6 drachms; the Enfield ball weighs but 1 oz. 2 scruples.

grene, and forcing the surgeon, if the injury be to a limb, to have recourse to the last means of saving life, to amputation.

In the treatment of the wounded among the French it was found necessary to have recourse at an early period to good, strong meat soups, roast meat, and generous wine; Port wine, above all, was used with great benefit.

As regards the local treatment of wounds: the immediate extraction of foreign bodies was always the general rule with the French surgeons. In cases of hemorrhage, the ligature was used for the obliteration of large vessels; astringents and cauterization for hemorrhage *en nappe*. Of all astringents, the most efficacious were solutions of the perchloride of iron and of the persulphate of the same metal. These salts, Dr. Scrive says, are worthy of being introduced into the materia medica of armies. The favourable results that followed the practice of removing constriction by long and deep incisions demonstrated the real benefits of the preventive dilatation of wounds. This practice, formerly so much insisted upon, has been generally abandoned of late years. Its advantages in remedying strangulation, excess of inflammation, the stagnation of pus and traumatic gangrene were seen in numerous instances.

The experience acquired in the Crimea on the interesting questions concerning amputations shows that, in war, these means of last resource must be largely applied. "If," says Dr. Scrive, "you hesitate in cases that appear doubtful, or if you place yourself a little too much on the ground of what is called conservative surgery, you are not long in repenting of it, and in seeing wounded men die in consequence of wounds that often amputation would have been able to save. Too often we made this sad experiment." (P. 461.) The same is the opinion of the English surgeon, Macleod, who says that it can hardly be doubted that one main cause of that mortality which attended compound fractures of the limbs was the great striving after conservatism, which influenced all the surgeons of the British army.

Primitive operations were the most successful in the French service, and the proportion of their success surpassed sometimes one-half, sometimes two-thirds, the favourable results of consecutive amputations. The experience of the English in favour of early operations was also, according to Macleod, unequivocal, and such as to require no illustration.

The experience of the Crimean war, both in the English and the French armies, is therefore most decidedly in accordance with the teachings of Hennen. This excellent surgeon says: "*The sum of human misery will be most materially lessened by permitting no ambiguous cases to be subjected to the trial of preserving the limb;*" and, moreover, as respects the time of operating, *operate with as little delay as possible.* The Italics are as we find them in the original edition of Hennen's work.

Dr. Scrive makes one exception to the rule of primitive amputation, and that is in the operation at the hip-joint. This he is convinced is always fatal if practised primitively, by which he means within two or three days after the receipt of the wound. In nine cases where it was performed primitively, the patients all died at the time of the operation, a few minutes or a few hours afterwards. In three cases where the operation was performed consecutively, one lived five days, one twelve, and the third twenty, and under better conditions he is satisfied that the patients would have entirely recovered.

Resections were tried a very great number of times. Generally they did not succeed, with the exception of a certain number of those practised upon the upper extremity, and principally the resection of the head of the hu-

merus, which seems to enjoy great privileges in this respect, and to furnish frequent examples of success. All those tried on the lower extremity, even in the diaphysis of the bones, generally failed; and the same is true of the greater part of the trephinings of the cranium. The experience of Macleod in the use of the trephine is the same; of twenty-eight cases in which it was used in the English army, twenty-four died.

Of chloroform, Dr. Scrive says that the benefits derived from its use were immense. Although it was used in thousands of cases, it never caused the slightest serious accident. Not only was it used to obtain complete insensibility in every important operation, but also to render more supportable the pain from wounds altogether desperate, and to calm nervous and irritable persons, the dressing of whose wounds occasioned much suffering. A special apparatus was employed in its administration, by which the dose was exactly measured, and that allowed a proper intermixture of air and chloroform. When insensibility was produced the apparatus was withdrawn; complete relaxation of the muscles was never reached. Given in this way, with reserve and prudence, to insensibility and no further, Dr. Scrive states that, far from adding to the general stupor of a patient severely wounded, it excites favourably the depressed nervous system, rouses up the circulation rendered slow and weak by the general shock, while destroying at the same time the enervating sensation of pain.

Great resistance of the wounded to anæsthesia a day or two after the reception of the injury was frequently remarked. A much larger dose of the chloroform was necessary before insensibility was produced, and the period of excitement was so strongly marked that several men were often required to hold the patient. To avoid this over-excitation produced by traumatism, the rule was observed to amputate as much as possible the very day of the wound. When consecutive amputation was necessary from gradual sinking from suppuration, anæsthesia was effected with the most perfect tranquillity.

We here conclude our task of endeavouring to extract from this valuable work of Dr. Scrive that medical and surgical information which seemed to us most useful and applicable, both directly and indirectly, to our country in the present condition of affairs. We might have made many and most just animadversions in regard to the manner in which the sanitary condition of our volunteer troops has so far been attended to, but all this we leave to others more competent and more willing than ourselves to perform the task. W. F. A.

ART. XIV.—*A Treatise on Diseases of the Joints.* By RICHARD BARWELL, F. R. C. S., Assistant Surgeon, Charing Cross Hospital, &c. Illustrated by engravings on wood. Philadelphia: Blanchard & Lea, 1861. 8vo. pp. 463, with index.

THE author of this work has admirably satisfied a want which has long been seriously felt—that of a treatise on diseases of the joints, as he expresses it, "equal to or rather beyond the current knowledge of the day." Some familiarity with the London publication, of which the copy before us is a faithful and handsome reprint, has convinced us that Mr. Barwell's pages fully justify the modest hope expressed in his preface. Not only do

they supply the acknowledged deficiency in their ample exhibition of the current knowledge, but they have gone decidedly beyond this in originality of views, and in methodizing and illustrating matters of pathology and therapeutics which have not hitherto been brought within the reach of professional readers, especially in so clear and practical a form, except in detached and fragmentary essays.

His treatise is the result of his well-known practical devotion to the subject, under the best influences and opportunities, during a period which has been distinguished for material progress in the comprehension and management of articular diseases; and it has the benefit of his own investigations into the physiological and the morbid anatomy of joint structures, which have already established his reputation in connection with the study of these important affections.

The leading and most meritorious characteristic of the book is its thoroughly practical and straightforward mode of conveying instruction. The author alludes to his anatomical researches and to the consequent demonstrations given, as a part of an indispensable course of investigation preliminary to the great object of inquiry; but the real weight of his work, he tells us, "has been at the bedside, and the greatest labour devoted to interpreting symptoms and remedying their cause." This combination of clinical and anatomical observation, extended as it is, and enlightened with much careful reading drawn from a wide range of sources, would render Mr. Barwell's treatise an acceptable one upon any more familiar theme. In the present instance it has enriched our literature with an unusually valuable contribution upon a difficult and always exacting branch of surgical science and art, in which such a reliable and candid regulator, in the midst of great advance, has been sadly needed on both sides of the Atlantic.

Chapter I. is occupied with the physiological anatomy of the joints. The whole subject is clearly and amply exposed and illustrated in the course of some twenty-five pages, which afford a very useful introductory to the subsequent chapters. Of these, the first eight are devoted to the consideration of the affections which involve the lining membranes only of the joints, and constitute, with the preliminary chapter, the first of the two parts into which the volume is divided.

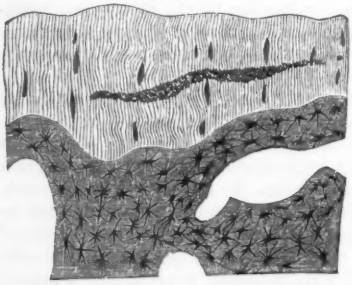
The second part, consisting also of nine different chapters, treats of the diseases of the joints which have their origin in the bones. Under this general head we find the most interesting class of joint affections, although some of them, such as hip-joint disease, can hardly be regarded as invariably "commencing in the bone." The division is one of mere convenience, however, as it is one of the advantages of the volume, that the author is not bound by logical requirements at the expense of practical utility. He does not hesitate to treat of coxalgia in the latter portion of his book, although it may sometimes commence as a synovial disease; and he devotes a special chapter to this affection, for the sufficient reason that the separate discussion of the diagnostic signs of hip-disease would increase the value of his book.

The chapter on the physiological anatomy is extremely interesting. Without dwelling upon unnecessary details, he omits nothing that is essential to a thorough understanding of the peculiarities of structure, as he views them, and applies them in his subsequent expositions of the pathology. These views and applications are based upon independent and sometimes highly original observations. They are ably sustained, and are entitled to great respect, notwithstanding their occasional variance with the current

theories. We can only glance at some of the more prominent. First, in regard to the structure of the articular lamella, previously described and represented by the author in his paper on the subject in the *Med.-Chir. Review*, for Oct. 1859. We quote his language, with the marginal engravings accompanying the description.

"It in reality consists of a series of very minute parallel tubes, which run in a wavy course from the bony to the cartilaginous surface. Among these, but having no special, if any, communication with them, are the bodies mentioned by Kölliker as undeveloped bone cells. In some sections —those, namely, which are not made quite parallel with the axis of the joint from which they are taken—the tubes of the articular lamella cannot be made out, but the section is minutely dotted, from those tubes having been cut across. Fig. 3 is a tolerably successful representation of this structure. It will be seen that certain portions of the articular lamella are rendered darker than others, and this is a condition very difficult to account for; yet perhaps I may pretty confidently affirm that it arises from the tubular structure having become so bent in those lines that the canals have been cut through, giving a brown, darker, and finely-mottled character to that part. This structure is similar in every mammal in which I have examined the lamella, but perhaps it is plainer in the rabbit than in any other I have yet seen.

Fig. 3.



Cancellous structure and articular lamella from upper end of the human humerus, showing tubular structure of the lamella, magnified 500 diameters.

"Having thus succeeded in ascertaining the structure, as seen laterally, it seemed advisable to view the same part from above; for this purpose a joint end, with as flat a surface as possible, was chosen; either end of the tibia in most animals answers this purpose sufficiently: the cartilage being scraped away, a small piece of the articular lamella was detached, and ground thin enough to be transparent. In this view the black spots or undeveloped bone-cells are less elongated, all the rest of the section is studded with dots, which, under a sufficient power, and where the section is very thin, appear as small round holes. This structure is the same in all animals that I have examined. (Fig. 4.)

Fig 4

Articular lamella, section parallel to surface, from lower end of human tibia, magnified 700 diameters.

"Thus the articular lamella is not an impervious material; but perhaps this account, which I have allowed to stand from my first paper on the subject, might have produced less opposition had the wavy lines which run through the section been named something different to 'tubes.' As far as the bony material is concerned they certainly are tubes, but I do not, and never did, conceive them to be lined by a tubular membrane—probably by compound fibre, or whatever form of fibrous material the altered cartilage may take. The

channels, running from the deep or osseous to the superficial or cartilaginous surface of the lamella, permit the passage of nutrient fluid from the bone to the deep surface of the cartilage. This is the one important point—it is immaterial what the channels, through which that fluid passes, be called, whether tubes, osseous intervals, or any other name, as long as it be fully understood, that there is a mechanism permitting fluid to permeate the structure and to nourish the cartilage from its deep surface.

"That part of the epiphysis, which remains unossified towards the joint cavity is thus separated from the ordinary bone tissue, and now becomes articular cartilage. It is not, as it has often been said to be, attached to the bone, but it is continuous with it: it is part of the same thing, one portion having received a deposit of lime, the other portion not having done so." (pp. 25, 26.)

He does not believe, with some observers, in the vascularity even of fœtal cartilage; nor is he able to discover epithelium overlying the cartilaginous surface. The only source of nourishment and growth is the attached surface through the articular lamella.

Next, as to the synovial membrane, the arrangement of which has been much disputed:—

"Some persons have thought that it was prolonged over the face of the cartilage; others, that it went between the structure and the bone; and some even have supposed that it was split, and that a lamina was placed in either situation. Some believe that the membrane is prolonged over the cartilaginous surface until the movements of the joints wear it away. My own conviction is, that at no period is the cartilage covered by synovial tissue, but that the membrane becomes lost very near the edge of the cartilage. In no young animal will any structure be found like a fibrous, or even a homogeneous membrane on the cartilaginous face, and the last layer of cells cannot be regarded as appertaining to a prolonged synovial membrane, since it is very evidently the result of a gradual process of change, and is the arrangement towards which all the corpuscles and cells tend even from their very deepest layers. Histologically, the synovial membrane and the apparatus of bone and cartilage previously described are exact parallels; the former consists, as just related, internally of cells, then of a fine membrane permitting fluids to pass through from a vessel-bearing subserous tissue. The whole cartilaginous apparatus consists of cells (with an intercellular substance), then of a fine lamella, permitting fluids to pass from a vessel-bearing tissue. These structures, or rather set of structures, being analogous, it is hardly conceivable that nature should permit such a tautology as to prolong the one over the other." (pp. 29, 30.)

The last point which we are able to notice is the theory of the synovial vacuum, based on the experiments of Weber. These experiments are criticised by the author, and successfully controverted by four experiments of his own, which are described in full. He also cites a curious case observed by him of a man who had an opening into the synovial membrane of the elbow-joint, through which the air passed in and out with the motion of the forearm; and yet, notwithstanding much hard work and climbing as a sailor, he exhibited no tendency to dislocation.

"The whole vacuum theory is untenable; the only fact which at all resembles it is, that cohesion of contact takes place between the joint surfaces, but this is not a vacuum: a vacuum is a space containing neither air nor other material—the cohesion we speak of takes place when there is *no space* between the parts interested.

"The fact is, that every joint has some special means, which hold the bones forming it in close contact, and such aids as atmospheric pressure may yield are but slight in comparison with these. Certainly no man gifted with a tolerable appreciation of cause and effect could regard the enormous power of the muscles passing from the scapula to the tuberosities close to the head of the humerus without considering that their tonicity alone would be of large effect in

keeping the head of the bone close against the glenoid cavity. Any one attempting to resect the head of the humerus in the dead subject cannot fail to remark their effects, even as lifeless flesh; and moreover he will find the ligaments and the tendon of the biceps and other parts of the greatest importance. Again, if the rotators of the hips and their direction and attachment be considered, their importance will not be slightly regarded, and besides these the psoas, iliacus, and glutei, in fact every muscle attached to femur and pelvis, have the same effect. Add to this the resistance of the cotyloid ligament, which forms a smaller circle than lies within the cavity, and therefore clips in the bone like a circular clamp, and we have quite enough to account for the difficulty in dislocating the bone.[1] Besides, there is the round ligament, which in certain positions takes considerable part in holding the head of the bone in the acetabulum.[2] The knee, elbow, all the joints in the body, have arrangements either of muscular force or ligamentous resistance for keeping the articular surfaces in contact. By this means only, viz., actual contact of the bones, can any assistance from atmospheric pressure be obtained. A vacuum, *i. e.*, a space unfilled by air between the bones, could not be maintained: either the surrounding parts must be forced into such space or the bones would be squeezed together, for there is nothing to keep them asunder, and so the space would be annihilated." (pp. 35, 36.)

Acute synovitis in its various forms, the pathology, symptoms, and treatment, are well discussed and demonstrated in Chapter II. We do not remember a monograph on any form of disease which presents a better exhibition of its subject, or a greater amount of pathological and clinical instruction. The very first paragraph is worth quoting on account of the comprehensive view it gives.

"A joint is, as the preceding chapter has shown, a very complicated apparatus, constructed of different tissues of different qualities, hard, soft, and elastic, serving different purposes. Yet they are all reducible to one form or class of tissue—the connective. Those different parts are so closely placed, and are so interdependent, that one cannot long remain sound while the others are undergoing any active morbid change. But some of the structures are more liable to disease than others, as indeed it must happen that those parts which undergo much action are, *cæteris paribus*, more likely to get some of it deranged: and such derangement is more likely to set up rapid disease than when the action is very slow, as a railway train going at the rate of sixty miles an hour is at least six times as likely to meet with an accident during that hour as one which goes at ten in the same time, and that accident is more likely to be severe. We shall be able to show clearly that the structures which are liable to primary diseases are the synovial membrane and the bones. From either of these points the malady may spread to every other part of the joint; therefore, after either has been affected for a certain time (synovial membrane or bone), the other becomes involved. Hence neither synovitis nor osteitis are utterly different throughout all their stages, because in the far advanced conditions the one disease will be more or less mingled with the other.

"The present chapter will be devoted to primary acute synovitis: a disease which perhaps acknowledges more causes than any other simple malady in the range of pathology, and which therefore may be divided into an almost indefinite number of sub-classes, each with its proper expletive affixed. Perhaps the clearest way of showing this would be to relate one or two cases."

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[1] "In my Experiment II. the weight hung on the pulleys produced a force sufficient to overcome this ligament and open it out; subsequently, therefore, whether or not air was admitted into the cavity, the head of the bone fell out on the application of the force."

[2] "The reader is referred to a paper by Dr. John Struthers, in the Edinburgh Medical Journal, vol. iv. 1858–9, p. 43, for a particular account of the use of the round ligament. It appears that the position *par excellence* wherein this band may act as a check ligament, is rotation outwards with flexion. It is rendered nearly tight in adduction with flexion, and in adduction with rotation either inwards or outwards; but only in the first-named position does it bear the strain."

He then reports five different cases in detail, each from a different cause, and with different general symptoms, the inflammation of the synovial membrane being that alone which they have in common, although, locally, the morbid action is, up to a certain point, very much the same in each.

The sixth case related is a rare instance, in which death from injury afforded an opportunity to examine pathologically into the changes produced by a simple acute synovitis in the early stage. The man died on the fifth day. Mr. Barwell made a careful observation of the knee, and describes the appearances in the following words:—

"The joint contained about an ounce and a half of synovia, which was slightly turbid, opalescent, and in which floated shreds of false membrane, some semi-transparent, others opaque and white; these latter being more evidently fibrous; one of the shreds was loosely attached by one end to the synovial membrane, the rest floating freely in the fluid. The whole membrane was intensely injected, the vessels not being on the surface, but as though visible through the film; in some parts the injection was much more violent than in others; the deepest in colour were the fringes around the patella, the so-called alar ligaments, and the subcrural cul-de-sac; in many spots of these parts were actual extravasations, which, again, were not on the surface, but beneath a fine film. In the less injected parts of the membrane long tortuous vessels could be distinctly traced. The surface of the membrane itself was finely roughened; on holding it up and looking towards the light over the surface, it was seen to be covered with papilla-like or velvety elevations; it looked like the surface of the duodenum when the valvulæ conniventes are straightened out. The membrane was easily torn, and very easily stripped from the underlying tissue, which was highly injected and infiltrated by a turbid serum.

"On the inner condyle of the femur was a spot as large as a sixpence, whence the cartilage had entirely disappeared; the edges of the ulcer were perfectly smooth, sharp, and clean. The rest of the cartilage was entirely healthy." (p. 50.)

The section on the treatment, like all the rest, is admirable and full of practical hints. We can only refer to his remarks on the resort to free incisions in suppurating joints as advocated ten years ago by Mr. Gay, and long before by Petit, Boyer, and others on the continent. Mr. Barwell cordially approves of this plan and refers to the French authorities in support of it, giving the usual reasons for regarding it with favour, and insisting on free and always dependent incisions. There has, doubtless, always been more or less hesitation, even among well informed surgeons, as to opening suppurating joints, and this still exists notwithstanding what has been said in its behalf for some years past; but we are glad to see Mr. Barwell distinctly referring to the practice as an old one, since it has been claimed in this country as a recent improvement. We remember it as regular practice in France, at least twenty years ago; and years since have heard it advocated, on our side of the Atlantic, as not only safe, but the only rational method of dealing with the articulation in a state of abscess.

Chapter III. is a short one on acute rheumatism, the chief object of which appears to be to show from the local changes, as well as other data, that the local affection of the joints in this disease is inflammatory. In this he successfully combats the dogma of Dr. Todd, that "rheumatic fever is not symptomatic of local disorder," so far at least as the joint affection is concerned, by careful reasoning as well as by the more decisive logic of morbid anatomy. Five cases are reported, in which the local examination sufficed to prove the inflammatory nature of the changes, and to illustrate the morbid anatomy of articular rheumatism.

"These cases suffice not only to prove that the disease in the joints is inflammation; but also to illustrate its morbid anatomy, viz., inflammation of, and

effusion into, the synovial membrane, and the periarticular tissues, followed by thickening of those parts. In severe cases the effusion becomes opalescent, even puriform, and the cartilages partake in the inflammatory action. The metastatic quality of this inflammation is very remarkable, but the description given of the sudden disappearance of all morbid symptoms is usually exaggerated; for although there can be no doubt that the inflammation itself changes places with extreme rapidity, it is not true that the part lately affected is left in a perfectly healthy condition; on the contrary, considerable swelling still remains, and disappears more or less slowly according to the greater or less amount of effusion and thickening. On the symptoms of this disease I shall not enter, as it belongs to the physician; but it is well to observe that the surface of the joints are redder than in any other form of arthritis, showing that the areolar tissue is more widely involved, so that even the subcutaneous layers are inflamed and the effects do not always disappear with the fever; but sometimes a joint, generally a large one, as the knee or the elbow, is altered, and requires considerable attention and care; sometimes, even in spite of all that art can do, the limb can never be recovered." (pp. 88, 89.)

Pyarthrosis, strumous synovitis, rheumatic synovitis, other forms of chronic synovitis, including syphilitic, gouty, and simple, hydrarthrosis, and loose cartilages in the joints, complete this portion of the series of synovial disorders, and are duly considered in the remaining six chapters. These different articles are all worth a careful study, and contain so much that is practically useful as well as philosophical, that it is difficult to particularize. We have been most interested in the chapter on strumous synovitis as especially exhaustive and lucid in its pathological and therapeutic reasoning, its illustrations and description of the structural changes and appearances. Were it the purpose of this article to present a regular analysis of Mr. Barwell's treatise, or to attempt anything more than to interest our readers in his work, by giving some idea of its high character and the peculiar value of its contents, we should feel obliged to dwell upon this portion of his pages as among the best of the book. The amount of positive instruction in regard to scrofulous diatheses and diseases, generally, contained in this chapter, its graphic portraitures and sensible directions for the treatment of these ever-dreaded forms of human ailment, ought alone to secure the grateful attention of American practitioners; since there is not one who can escape the responsibility under which its precepts and demonstrations, and individual prescriptions, would afford him signal aid and comfort.

The nine chapters of the second part treat, respectively, of acute articular osteitis (in a general account), of strumous articular osteitis, chronic rheumatic arthritis (osteitis), inflammation and degeneration of cartilages, hip-joint disease, affections of synovial sheaths and bursæ in the neighbourhood of joints, hysteric pseudo-disease of the joints, the restoration of mobility, and conformity to crippled joints; and lastly, the removal of diseased joints.

Of these, the most important and interesting are Chapter XI. on strumous articular osteitis—which resembles the corresponding one on strumous synovitis in extent and character, and goes even beyond this in the completeness of its pathological details and comparisons, and the practical value of its therapeutic teachings; Chapter XII. on chronic rheumatic arthritis; Chapter XIV. on hip-joint disease; Chapter XVII. on the restoration of crippled joints; and Chapter XVIII. on the removal of diseased joints.

In discussing strumous articular osteitis, the difference between caries and necrosis, and the peculiar changes which characterize them, the course of

scrofulous inflammation in the bones and adjacent structures, and the attendant muscular spasm, are described with great care and fulness. Under the head of symptoms are particularly well shown the diagnosis, first, between strumous synovitis and osteitis; secondly, between necrosis and caries in the joint end of a long bone during the earlier stages; and lastly, between necrosis and caries in the joint end of a long bone during the later stages. In each class the differential diagnosis is summed up in a tabular form, so as to present the distinguishing points at one view.

A good idea of the author's practical discrimination may be formed from the following paragraphs, in which he particularizes in regard to the mode of dealing with strumous osteitis in different constitutions.

"Sir B. Brodie, from whom no writer of the present day would willingly differ on the subject of joint disease, has discussed the subject of treatment too generally, and his remarks refer, I believe, to a further advanced period than the early one, now under our consideration. The earliest condition (marked by mere pain, reluctance or inability to move the joint, and some not easily perceptible swelling, while yet there is no muscular spasm or starting pains at night) should, in all but the most feeble constitutions, be treated, I am quite sure, more actively than he has advised. Two classes of cases come under our notice—the one very chronic, the other less so; either may occur in delicate, and pale, or in fat and florid, children. Such circumstances make a difference in the kind, but scarcely in the vigour of the treatment to be adopted. At p. 129, two forms of struma were described; at p. 147, the treatment best adapted to each. Now, although either strumous state may light up an inflammation in any tissue of the body, the slow, long-pending osteitis belongs especially to the coarse clumsy condition of struma, as a deep sluggish ulceration of the cornea, with congestion of neighbouring parts, is an accompaniment of that form of the disease. But inflammation of an epiphysis may also be developed by the struma with fine connective tissues, yet such disease is more rare, more active, getting either well or worse more quickly. It is comparable to the superficial, rather ephemeral and non-congestive inflammation of the cornea, which leaves a thin but expanded nebula on the surface. Let us take our example of treatment from what we see of its effects on the eyes. The patient comes with the eyes watering and discharging a thick Meibomian secretion, holding down his head, and dreading light beyond everything. A brisk but not drastic purge with mercury is administered, and when it has acted and brought away the thick mucus that hangs about the lining of the intestine, he is able to look at the light without much difficulty, and the brunt of the disease is past. It has been my fate to see cases, that hung about for weeks, and whose treatment had been judicious, except that from want of a clear intestinal surface medicines could not be absorbed. Such a purge has been always productive of benefit, and allowed the medicines, perhaps the same as were previously useless, to have their proper effect. If, then, a child be brought to the surgeon with the early symptoms of articular osteitis, if he be gross, somewhat coarsely moulded, and have the discolorations of skin described at p. 130, he should be treated by an immediate purge containing mercury, which is to be repeated if necessary.

"This simple means, though strongly insisted upon, is not intended to be held up as a cure of early osteitis; it is the mere initiative. Much has been said against the administration of mercury in strumous disease: probably the wholesale condemnation of the medicine is intended only to apply to its use when pushed far enough to touch the gums. Let me again illustrate the applicability of the remedy by referring to strumous ophthalmia in a later stage than when it was before used as an example—when the cornea has begun to be dull in one or more spots; beginning in fact to ulcerate. I have seen treated, and formerly have myself treated such cases, with iodide of potass, cod-liver oil, iron, quinine, and other tonic and antistrumous remedies without avail; then, by the administration of a few very mild mercurials, have stopped the tendency to ulceration, or have healed ulcers already produced. In the same way as before we may apply the remedy found useful for scrofulous inflammation in one place to the

same disease in another. The prescription which I generally use in this form of epiphysal inflammation is one grain, or, for strong children, two grains of gray powder, with one grain of quinine, night and morning: such a dose I continue during two, three, or even four days, then give the quinine alone at the same intervals. The mercury should never be pushed beyond a slight alterative effect, and is not to be repeated unless the skin and eyes again become muddy and thick in hue. Very many early inflammations of a joint-end in this particular constitution come under my care in the course of the year, and I have no hesitation in recommending the above mode of treatment as capable (combined of course with suitable local means) of checking the diseased action in a large number of instances.

"Strumous persons, with thin skin, clear bright complexion, and finely-cut features, are not benefited by mercury, even in slight alterative doses; neither is a purge, as above described, required so constantly, as in the coarse-conditioned struma, and such remedy must be of a non-irritating description. Cases in which a bone inflammation is developed in the delicate-formed struma are marked by a higher degree of pallor and debility than is present in any other form of commencing scrofulous disease—except, perhaps, in a rapid species of phthisis. The inflammation itself is less chronic in quality, and tends to pass more quickly into suppuration than the sluggish inflammation of the other form of strumous disease. The local treatment should be more decidedly and actively counter-irritant, but as little debilitating as possible; *e. g.*, blisters allowed to heal at once, and repeated on various parts in the neighbourhood of the disease, or the actual cautery; while all such remedies as issues or setons, that keep up a continuous and debilitating discharge, are inadmissible. Of cod-liver oil, quinine, and steel, we need not speak more especially here. Their great value in this form of strumous disease was pointed out at p. 143." (pp. 258, 260.)

His remarks upon the mechanical treatment of these affections are very precise and full. They are quite *au courant* in their minuteness of detail and in principles and practice; and although not in advance of what has been well established in this country, they bring the accepted methods of combating these formidable diseases at once within the reach of every reader, in a shape which all can easily comprehend. It would be interesting to discuss his views upon the modes of overcoming the deformity from active contraction of the muscles, or passive "contracture," such as the use of tenotomy, of immediate or gradual extension, and of the various appliances intended for these purposes. Mr. Barwell prefers the interposition of gum-elastic springs in effecting the extension and counter-extension now adopted. The following is the apparatus which he claims to have invented.

"The principle of its construction is to make a strong India-rubber spring, or accumulator, act as both extending and counter-extending force. For this purpose it is fastened by each end to a piece of catgut that plays round pulleys, attached to either end of the splint. I will describe particularly the arrangement for the knee. A long Desault's splint is furnished at its upper part with a loop of strong wire, or of steel (A), which carries a small pulley, and which projects outwards about an inch and a half. The lower part is provided with a bar running across the space of the notch, and also carrying a pulley (D). From the lower end of the splint, projecting inwards an inch or an inch and a half, is another loop, carrying a third pulley (E). A perineal band (B), passing round the upper part of the limb and splint, has a piece of rather thin catgut (violin string A or D) attached to it, which going through the upper loop of wire runs round the pulley (A), is brought down on the outside of the splint, and is attached to one end of the India-rubber accumulator (C). Round the foot and ankle are fastened two pieces of webbing (F), which lace over the instep, and to both sides of which is sewn tape, forming a loop below the sole of the foot.[1] This

[1] "A broad well-padded leather strap going round the ankle is as good, and strapping plaster, as described in Chapter XIV., is better than either. A few little con-

tape affords attachment to another piece of catgut, which plays over the pulleys, in the lower part of the splint (E and D), and is tied to the other end of the accumulator (C), with the fitting amount of tension.

Fig. 16.

"This splint may, by fitting modifications very easily applied, be adapted to any joint. It is very useful when it is desired to produce gradual extension of a joint, as of the knee; or, when properly adapted, of any other articulation. The ordinary plan of attaching a weight to the end of the foot, for extension, and fastening the patient by cords to the upper part of the bed for counter-extension, does not answer, as it produces such weariness, that no person can bear the constraint long enough to allow of much benefit. This mode of getting extension and counter-extension on the splint itself, confines nothing but the joint, and even that to as slight a degree as may be desirable; for instance, in the case of a slightly bent knee, it is not absolutely necessary to bandage the apparatus to the limb; the force of the spring keeps it sufficiently in place. It is important only that the anklet and perineal band should be duly padded and kept clean.

"Besides the mere power of straightening a bent joint (only a secondary use of the splint, and not that for which it was invented), the India-rubber spring counteracts that force, which presses the bones too violently together, thereby producing the spasm, keeping up the irritation and the caries. I have seen the most violent pains yield gradually to this contractile force: it appears, by its unvarying, constant, and yet not unyielding power, to tire out the muscles, to overcome their spasm, and to keep the joint surfaces, if not asunder, still not pressing together. It is, however, necessary that all the straps and bands be padded, and fitted with the nicest care." (pp. 268, 269.)

Our next quotation must be from the chapter on hip-joint disease. After speaking of the difficulties attendant on the study of this most embarrassing affection, and the various opinions in regard to the true seat of the disease in its commencement, he proceeds to show, conclusively, we think, that it "may begin in the synovial membrane, and in the bone, like other joint-diseases; there is no reason to suppose, nor is there the slightest proof, that it may commence in structures in which diseases of other joints do not begin, for instance, the ligaments."

"We have seen that when the subsynovial tissues, in which ligaments are placed, inflame, the ligaments themselves suffer, soften, and become thickened or absorbed as the case may tend; we have seen that internal ligaments, as the

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trivances will be found to aid in the ease with which the apparatus may be adapted. For instance, extension need not be made on the perineal strap when the splint is bandaged on the limb; but the accumulator may be fixed to the upper part of the wooden splint. There will be some tendency to ride upward, owing to the mode in which the catgut passes round the lower pulley; hence the perineal band should be fastened to the splint, and the tension on it will be very slight. (See Chapter XIV.) It will be found convenient to have the accumulator near the upper part of the splint, and to fasten to the lower catgut a piece of tape which can be readily tied and untied from the ring in the India-rubber. This being a treatise upon a special subject, the author has no right to turn to another, but he must point out the value of this extending force in fractures at the upper part of the thigh. In caries of the vertebræ some modification of it may be found useful."

crucial of the knee, which are surrounded by folds of synovial membrane, and which are in the position of a subsynovial tissue, are more apt to follow quickly in this course, than external ligaments; but the disease is not the less a synovitis, because there happen to be internal ligaments which participate early in the inflammation and become quickly changed or absorbed. How very frequently does it occur in the knee-joint, that the crucial ligaments have entirely disappeared, even in cases of subacute synovitis, where the cartilage is hardly altered, and where the general change in the synovial tissues is but slight! I have also seen the crucial ligaments all but converted into gelatinous structure, their presence being only marked by a few white ligamentous lines, running parallel to each other through the pink mass. In the same way, the ligamentum teres of the hip-joint has been found to be much softened and inflamed, while the synovial membrane around it is red and hyperæmic; but there is no possible reason for assuming that the disease begins in an inflammation of that structure more than in any other part of the synovial and subsynovial tissue. Other authors have located the diseased action in the fat which lies in the bottom of the cotyloid cavity; but this opinion is too antiquated, and too far behind the science of the present day to need serious refutation. Again, the cartilages of the hip are not differently situated to those of other joints in regard to their nutrition and diseases; they are as often, or indeed may be more often than in other joints, the seat of degeneration (p. 290); and in old subjects, in whom during life no symptom of joint disease existed, such degenerative ulcers will very frequently be found; but, except this condition, which does not produce a detectable joint disease, there is no malady of the hip commencing in the cartilage, any more than there is a malady of other joints beginning in that structure.

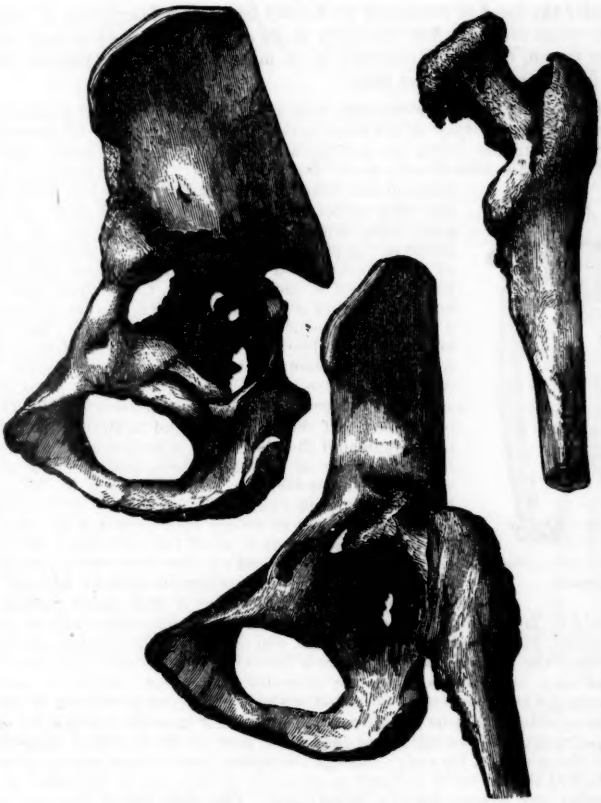
"Thus we come again to the two tissues, viz., synovial membrane and bone, whose inflammation is, as we have seen, the cause of disease in other joints; and I am sure of truth in asserting, that every hip disease commences in one or other of these parts." (pp. 297, 298.)

We cannot follow our author through this most instructive history of coxalgia. It will probably be referred to by all classes of professional readers as the most interesting if not actually the best in the book; and as such we commend its pages to those who may see this notice of it. The leading points of semeiology, pathology, and treatment are severally considered in a masterly style. The history of the different stages, the changes of shape and position, the non-reality of the apparent shortening—which he proves by a series of experiments—the causes of pain at the knee, the action of the muscles, the actual shortening, the effects of articular pressure, the rarity of spontaneous dislocation and the diagnosis of this displacement, the course and localities of abscesses and their openings, the puncture of distended capsule, the treatment by extension, and many other matters are elaborately discussed.

The only complaint we have to make is that Mr. Barwell neglects to acknowledge the claim of our countryman, Dr. March, to the merit of arousing attention to the rarity of spontaneous dislocation, to the effects of articular pressure, and to the principle of extension and counter-extension, especially with exercise, which have done so much for the treatment of coxalgia. On these points high honour unquestionably is due to the intelligence and zeal of Drs. Harris and March, and their followers, Drs. Davis, Bauer, and Sayre, of this country. Before leaving this portion of the book, let us present a beautiful illustration of a diseased acetabulum and head of femur, in which may be observed the peculiar prolongation upwards of the acetabular cavity, and the alteration in shape of the head of the femur, which were long ago pointed out by Dr. March (*Trans. Am. Med. Assoc.*, 1853, vol. vi. p. 479) as the usual effect of pressure resulting from muscular irritation, but which Mr. Barwell refers to as a peculiar influence upon the continuance of the malady which had "escaped notice."

"Let it be observed how the cavity of the acetabulum has been prolonged about an inch upwards; how the head of the femur has been altered in form; how, also, in the position which in life they last assumed, the two fit accurately together, and how the track left behind became narrower as the caput femoris wasted. It is plain from this mere physical evidence that the head of the femur was used almost like a copperplater's graver, to furrow the cotyloid cavity up-

Fig. 22.



Diseased acetabulum and head of femur.

wards on the ilium, and has been almost worn out in the process. It has been held tightly to the floor of the cavity by the pyriform, obturator, and other capsular muscles, while it was forced upwards by those previously specified as abnormally contracted. Thus the acetabulum is made to travel upwards and also inwards, whereby an opening through the floor of the cavity into the pelvis is not unfrequently produced, as shown by the figure. I say, that such evident yielding to the pressure upwards is not an exceptional case, but is the rule: that when we find a hip-joint ulcerating in any other way and position, it is that some rare circumstance has caused a primary osteitis in that particular spot. It must also be remarked that as the head of the femur travels upward, producing, in

that part against which it presses so abnormally, ulcerative absorption, it causes beyond that point an additional growth of bone, forming a new lip to the new cavity (according to the law of increased growth and induration beyond the focus of a suppurative inflammation). It follows, naturally, that the change in place of the joint-cavity as in the head and neck of the femur produces a certain amount of *real* shortening, but this is slight—probably does not exceed an inch—yet adds to the difficulty in appreciating the occasional later and more violent effects of the disease." (pp. 313, 314.)

Under the head of treatment we have a figure and description of the author's splint for "the first and early stage of the disease, while there is as yet no spasm, no morbid contraction of muscles, nor any alteration, therefore, in the position of the limb."

"One portion made of wire-gauze, with a border of thicker wire, passes round the pelvis from one spine of the ilium to the other, its upper edge corresponding accurately with the crista ilii, and the whole being of such breadth, that the lower border lies immediately above the trochanter. Another portion on the diseased side runs from the end of this pelvic band down the outer aspect of the thigh beyond the knee; the two parts being immovably joined together, or cut out of the gauze in one piece. In order to secure a more lasting fit of the pelvic portion without making it too stiff, a little hook is placed at each of its ends, to which an India-rubber belt, passing across the abdomen, can be attached at a proper degree of tension. (See Fig. 24.) The angle between the pelvic and thigh portions must be such as will cause the femur to be extended on the pelvis (this will be an obtuse angle), as a flexed position gives rise in subsequent stages to certain embarrassments, which should be avoided. If the surgeon intend to apply any counter-irritants behind the trochanter, a portion may be cut away from the back of the thigh-piece, and lest this should weaken the apparatus too much, the back of the pelvic band may be connected with a lower point of the femoral portion by a piece of strong wire, or better by a piece of flat steel, so curved as to avoid the trochanter, and to fit the buttock. I have used all these forms with advantage; they keep the hip quite immovable without irksome confinement of other limbs, and if well fitted, padded, and fastened by means of a pelvis bandage, are a very comfortable form of splint. The gauze, with its edge of thicker wire, can be bent to the requisite form, and by help of the India-rubber band across the abdomen, retains a very perfect fit. In the more acute cases, it may be desirable, that means should be used for allowing the adult patient to be raised, without producing movement of the hip, so that a bed-pan may be placed beneath him. This is easily arranged, by means of a pulley fixed to the ceiling, or the upper part of the bedstead immediately above the middle of his body: through the pulley passes a rope, one end of which is attached to four cords fastened respectively to each end of the pelvic part of the splint, and to each leg by a broad band. The other end of the rope hangs within reach of the patient's hands. By pulling upon this part of the cordage the patient can lift his whole body, and the apparatus, from the bed.[2] A child too young to execute this manœuvre can be lifted so easily that no such apparatus is necessary." (pp. 321, 322.)

Fig. 24.

Splint for hip-disease in children.[1]

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[1] "The use of pulleys at either end of the thigh-piece will be explained hereafter. For full-grown persons I find it preferable to make the thigh-piece of wood, which must be fixed by screws or other means immovably to the pelvic portion. See Fig. at p. 326. The thigh-piece is represented as broken, and a part removed to avoid giving the figure too much length. The extending apparatus and perineal band are only to be used in a later stage."

[2] "This plan is borrowed from M. Bonnet."

In speaking of the second stage, with its nightly pains and spasms and more or less change of posture, he presents us with a figure of the extension splint above and previously referred to.

"During the whole of this second stage, the deformity is due only to muscular contraction from nervous irritation, and a very slight force, if continuous, will overcome this power. The essential is to obtain a *continuous* force. All the various forms of screws, pinions, and racks are not continuous when applied to a living organism; the former are fixed, the other is moving: so that at one time the power will be intense and rigid, at another will not be exerted at all. A spring is the only force which, while it yields sufficiently to the movements and struggles against confinement, which a living part is sure to make, yet exercises a constant, never-failing traction, sure in the end to overcome muscular force, without painful rending and violent stretching of parts. Any deformity from muscular contraction[1] at the hip may be restored to the proper posture by means of the extending splint, with the pelvic wire-belt. I will not detain the reader by describing the mechanism of pulleys, straps, &c., as it has been already done (p. 268), but the method of using the splint for this particular purpose must be explained. Let us suppose the patient lying on his back, with the affected limb bent both at hip and knee. The surgeon begins by applying a broad piece of strapping on either side of the leg, from the knee to the foot, allowing an inch or an inch and a half of the material to project below the sole; he then bandages firmly to the knee. The plaster ought to be spread upon strong cloth; it is for the purpose of making extension upon the limb after the American fashion, and is much more comfortable than any other mode. It is better to leave the patient some hours before any force is exerted on the strapping, that it may establish strong adherence. When it is supposed to stick sufficiently firmly, the splint is to be placed in position; the upper portion will pass round the pelvis, the lower lie along the bed, quite out of reach of the distorted limb. The surgeon now bandages from the foot to the top of the thigh, independent of the splint; arrived at the latter place, he causes the bandage to pass round pelvis and thigh, including all the upper portion of the splint, thus fixing it with sufficient firmness. Catgut is now to be fastened to the ends of the plaster projecting below the foot; the perineal band, properly padded, is to be adapted, and both to be fastened to the accumulator with the proper degree of tension.[2]

Fig. 25.

Splint for hip-disease.

[1] "The reader must be reminded that the word contraction in this sense only refers to an active condition: when muscles have been for some considerable time morbidly contracted, there supervenes a form of atrophy, accompanied by passive shortening and rigidity—contracture."

[2] "It is not always desirable to continue extension upon the perineal band when the patient is apt to chafe (until the deformity is overcome, the force must be thus applied); the upper end of the accumulator may then be fastened to the top of the splint. A glance at the mechanism will show that the force acts both upwards and downwards; but the lower pulley being in the situation of a movable one, causes the upward to be just double as much as the downward pressure on the splint. Hence, to prevent the apparatus riding up, it is necessary to use a perineal band, fastened to the splint, but the tension upwards will be equal to only one-

"For the first ten minutes, or quarter of an hour, the strain should be slight; the muscles soon after its application set up a startled sort of resistance, which, however, soon subsides, and then the India-rubber is to be pulled tighter. In a very few hours the foot or knee will have descended so much that a nurse, or other person in attendance, must tighten the spring, and in from eighteen to thirty hours the limb will have come down, and may be bandaged to the thigh part of the splint. This will have been effected without pain or violence; indeed, the starting pains previously complained of will even abate under the downward traction.

"If, however, the malposture be more fixed—that is, if the disease be further advanced into the second stage, the thigh cannot be thus drawn down without producing considerable pain; and in such case it will be better to give chloroform, and while the patient is under its influence, to draw down the limb into the proper position—namely, straight, and to bandage it upon the splint.

"The better to consider the treatment now to be adopted, let us glance at the condition of parts. The patient has had an inflammation in the hip-joint, and there is sufficient remaining (be it much or little) to keep up a destructive process; and at the same time he has morbid contraction of certain muscles, which under ordinary circumstances produce a deformity. We have overcome the deformity, and by bandaging can keep the limb in its place. This is the whole that M. Bonnet's, Dr. Bauer's, and all the other instruments at present used, profess to do; but, in my opinion, this is not enough. Binding the thigh to an immovable iron does not annul the contraction of muscles, but simply prevents the flexion or abduction of the thigh; the muscles still contract, and they drag the upper part of the head of the thigh-bone violently against the superior lip of the acetabulum. I have already (p. 312) pointed out that this pressure is the cause that hip-joint disease continues so long. It follows that, to enable the disease to get well, we should prevent this abnormal pressure.

"Upon this principle, our plan of treatment is simple. We have only to prevent the muscular spasm from pressing these two portions of bone together, and the disease will decrease; for in the majority of cases the pristine inflammation would be subdued by the time the second stage comes on; but that it continues in consequence of the pressure. The muscular contraction which pulls the thigh up must be met by another force which will pull it down. We cannot, nor do we wish, to separate the bones, but we can so arrange that the muscular force shall expend itself upon an external object, and leave between the head of the thigh and the acetabulum no more, perhaps rather less, than their normal amount of pressure. This can be done by the extending splint, examples of the use of which shall be given; these means will not *cure* hip-disease,[1] but they will place it in the best possible circumstances for getting well. The rest of the treatment must be adapted to the peculiarities of the case; thus, there may be tension of the capsule, evidenced by heat, considerable tenderness, and swelling behind the trochanter and at the groin, accompanied by deep-seated and confined fluctuation. A few rare cases occur whose rapid course of events would justify our including them under the head of acute synovitis of the hip, in which so much strong inflammatory fever is present that we may conclude the fluid to be synovia. By far the larger proportion of diseases in this joint belong to the subacute or chronic form; the slow succession of symptoms, and the irritable hectic-like fever, rather warrant us in assuming that the fluid in the cavity is pus.

"In the former instance, I would strongly recommend the application of the actual cautery, at the back of the trochanter, in two lines, so disposed as not to interfere with the application of the splint. A short line of cauterization at the groin, inside the femoral vein, has, in my experience, great effect in easing the pain, and also, I believe, in producing absorption of the effusion. If, on the other hand, the fluid in the joint-cavity be, judging from the symptoms, probably pus, we cannot hope to procure its absorption by such means, and any counter-

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third of the power of the accumulator.—Mr. Bigg, of Leicester Square, has undertaken to make these splints according to my pattern."

[1] "The possibility of curing a chronic inflammatory disease has been already doubted. See p. 265."

irritation will be not only useless, but positively injurious, by adding to the pain and irritable condition of the patient.

"We may, however, consider whether it will not be advisable to let out the fluid. It has been shown that, at a certain period of the disease, the capsule gives way, and the contents are poured into the surrounding parts; that a period of ease then supervenes, which is generally followed by abscess. We have, therefore, this warranty in nature for such a procedure, viz., that if we do not empty the abscess, it will evacuate itself; and by drawing out the pus by means of a canula, we prevent its diffusion among the muscles, and may even check, or very much retard, the formation of external abscess. Surgery is, I believe, indebted to Dr. Bauer for this operation, and it will, as far as my experience goes, be found very valuable. In my hands it has certainly relieved pain to a remarkable degree, and the formation of abscess has seemed, to say the least, postponed. But the more extended experience of Dr. Bauer himself will afford a more reliable account of its benefits than my smaller number of cases can furnish." (pp. 325–329.)

The apparatus of Mr. Barwell presents no especial novelty, but is easily constructed, and doubtless answers the purpose well. It is not equal in convenience and efficiency, however, to the splints of Drs. Davis, Sayre, Andrews, Taylor, and others in this country, which our author is unfortunate in not seeming to be familiar with. The absence of any account of the splint of Davis, and its modifications, and of their mode of use, is a positive defect in an American edition of the work. So much has been done by American surgeons towards the general progress in the study of the pathology and therapeutics of articular diseases, that we deeply regret in this instance the absence of the annotator whose labours it is the silly fashion now-a-days, under the lead of a few short-sighted journalists and domestic book-makers, to decry. The only reference to "American authors," except the occasional quotations from Dr. Bauer and some allusion to the resection statistics of Drs. Sayre and Kinloch, is an unjust one, which is doubly erroneous in basing its misrepresentation of the profession here on the mistaken assertion that Dr. Gross, in his *Surgery*, makes no distinction between acute and chronic osteitis. This is no very grievous wrong, and a not unnatural mistake in regard to the section misinterpreted; it is only mentioned as a curious fact, not to say illustration of the need, even to so complete a work, of a friendly word or two of explanation, in its new field of circulation, from those at least who know the ground.

As our object has been to enable the book to show for itself, in illustration as well as letter-press, its very superior claim upon the attention of our readers, rather than to discuss its many important topics, we pass very briefly by the remaining chapters. Two of these, nevertheless, are sufficiently interesting to be worth an extended examination by themselves, and are not exceeded in importance by any of their predecessors. They are the last two, on the Restoration of Crippled Joints, and on the Removal of Diseased Joints.

As an important adjuvant in the preventive as well as restorative management of incompletely crippled joints, he recommends an ingenious splint, which is applied in front of the limb, and enables the patient to move cautiously about with crutches or a stick. This consists of a thigh-piece and a leg-piece of wire gauze, connected together at an interval by a flat band of strong steel.

"The connecting steel portions were thus disposed: they were placed along the lower end of the thigh and the upper end of the leg-piece, and projecting beyond their edges, were riveted together so as to form a hinge at the outside and inside of the joint, in the situation of the respective lateral ligaments. The

splint in itself gives no support to the articulation; but two double hooks of an S shape are inserted into meshes of the gauze on the upper and lower portions, at such distance apart that an India-rubber accumulator can be suspended be-

Fig. 26.

Splint for flexed knee.

tween them at a degree of tension, which may be increased or decreased as circumstances shall require by moving one of the hooks into another mesh of the gauze. The power of the India-rubber forces the joint into a straighter position, gives sufficient support, and yet allows an amount of healthy movement, and therefore gradual re-establishment of muscular strength. No bad effects have followed its use: it is only necessary that the hinge be made of steel sufficiently strong to prevent the India-rubber drawing the two portions of the splint together, and so forcing the bones to press abnormally one against the other. When the apparatus has been worn some time, I adapt to the leg a pulley and rope, whereby the patient can flex the limb passively against the power of the India-rubber springs; by pulling upon the line and then relaxing it, action of alternate extension and flexion can be produced." (p. 377.)

The subjects of these last two chapters are discussed with the candid and cautious yet independent spirit, which characterizes Mr. Barwell's labours. The history of the excision of the various joints is fairly given, and the whole subject is thoroughly considered in its ordinary bearings. His statistical results, on the whole, are quite as favourable to resection as to amputation, and his verdict, although qualified and carefully expressed, with many practical cautions, is in recognition of the benefit which has already been derived, and must be looked for in the discriminating resort to these dangerous alternatives. He is in favour of resection only when, after a reasonable trial, the palliative treatment is evidently without avail. It is in this concluding chapter that we feel the want of further knowledge and experience more than elsewhere, but not through any fault of its indefatigable author; for there is not one that presents a more impartial, comprehensive, and particular exhibition of its subject in all the light that can yet be thrown upon it. Although there may be nothing new to the experienced surgeon and careful student in its summaries and precepts, it is none the less equal to the present state of the question, and affords the best view of that question, in its various important aspects, that has yet appeared in our language. E. H.

BIBLIOGRAPHICAL NOTICES.

ART. XV.—*Transactions of State Medical Societies:*—

1. *Transactions of the Medical Society of the State of New York, for the year* 1861. 8vo. pp. 408. Albany, 1861.
2. *Transactions of the Fifteenth Annual Meeting of the Ohio State Medical Society, held at Ohio White Sulphur Springs,* June 12th, 13th, and 14th, 1860. 8vo. pp. 267. Columbus, O., 1860.
3. *Medical Communications, with the Proceedings of the Sixty-Ninth Annual Convention of the Connecticut Medical Society, held at New Haven, May* 22d *and* 23d, 1861. 8vo. pp. 100. Hartford, 1861.
4. *Communications of the Rhode Island Medical Society, for the year* 1861. 8vo. pp. 66. Providence, 1861.

1. THE *Transactions of the State Medical Society of New York* open with the Annual Address of the President, Dr. DANIEL T. JONES, since deceased. The subject of the address is, the Medical Profession of the United States as it now is, and what it is destined hereafter to become: its mission, and its capacity to fulfil that mission, as shown by what it has already accomplished. With the sentiment embraced in the following remark of President Jones, we most fully concur.

"No one thing is more certain than that the science of medicine, as now taught and practised, will continue to rise above all the fallacious schemes and vagaries of a Pressnitz, a Hahneman, or a Mesmer. Its march will be steadily onward; the sound, educated common sense of mankind, that faculty which the politicians term the sober second thought, will weigh and try all those questions, and we have nothing to fear in a contest between enlightened and experimental science, and a blind experimenting faith."

The first scientific paper is on *traumatic tetanus*, by Dr. J. MCNULTY, of the city of New York. The position assumed and defended in this paper is that, in the blood of certain persons, there circulates a morbific principle, which produces in them a *tetanic diathesis*, which, when sufficiently developed, and then only, will give rise to tetanus, whether the patient shall receive or not any local injury. Dr. McNulty disclaims the ability to demonstrate the morbific cause of tetanus, or to show in what manner it operates to produce the result; these points he reserves for future investigation.

The chief argument in favour of the position referred to, is the unquestionable fact, that tetanus has occurred idiopathically without the infliction of any injury whatever, while, on the other hand, the very class of injuries, the infliction of which is reputed to be those most apt to occasion tetanus, occur daily, in a very large number of cases, and under all the conditions which are usually esteemed as those increasing their tendency to give rise to tetanus, without any tetanic symptoms being produced.

That there is in certain individuals a constitutional tendency to the occurrence of tetanus cannot, we believe, be denied; upon close investigation, however, it will be found, we feel persuaded, that this tetanic diathesis is dependent upon a peculiar condition—a morbid state possibly of the nervous system. We can see very little evidence of its being the result of a special tetanic poison circulating in the blood.

The second paper is on *the use of mercurials in acute pericarditis*, by Dr. S. OAKLEY VANDERPOEL, of Albany. The author denies that mercurials, given with a view to their specific effects we presume is meant, have any control over the march of the disease, or in the prevention of exudation; on the contrary, he believes them to be always injurious in consequence of the debility they produce.

This applies, however, only to the acute stage; the utility of mercurials in the chronic stage of pericarditis, when the system is relieved from the more immediate depressing elements of the rheumatic poison, the impoverished blood, or the exhaustive fever, Dr. V. does not call in question. The clinical experience of every one, he admits, proves that it quickens the absorption of any serous exudation, which may have passed to this stage.

The third paper is on *dermic medication*, by DANIEL P. BISSEL, of Utica. The dermic administration of remedies is not advocated by Dr. B. in preference to the usual plan of medication whenever this is practicable or successful. He confines it mainly to cases in which we are compelled to abandon the giving of medicines by the mouth, where often we are enabled by it to relieve troublesome symptoms, sometimes even to effect a cure in cases which without it must necessarily have terminated fatally. The endermic method of medication is no doubt often overlooked under circumstances where it is emphatically our only therapeutical resource.

The next paper presents the *statistics of suicide* in the city of New York for the years 1859 and 1860. It is by Dr. J. G. ADAMS, of New York city.

In 1859 the suicides amounted to 56—33 males, 23 females; being in ratio to the total mortality as 1 to 386; to the entire population, as 1 to 14,285. The individuals *found drowned* amounted to 165, of which 16 were females, and 56 foreigners. Two-thirds of these were probably suicides. There were 10 deaths from *unknown causes*, some of which may be reasonably attributed to suicide.

The 56 known suicides took place—11 cases in September; 8 in November; 12 in March and April, 6 in each month; in February and August, each 5 cases; in June 4 cases; in July and December, each 3 cases; in October and May, each 2 cases; in January, 1 case.

The highest mortality during 1859 occurred in the month of August, the lowest in May.

The ages of the suicides were as follows:—

Between 15 and 20 years . .	3 cases	Between 40 and 50 years . .	16 cases
" 20 " 25 " . .	6 "	" 50 " 60 " . .	5 "
" 25 " 30 " . .	8 "	" 70 " 80 " . .	1 case
" 30 " 40 " . .	12 "	Of unknown age . .	4 cases

Few cases occurred among women of the town, and none among a coloured population of about 10,000.

In 1860 there were 62 suicides reported—45 males, 17 females; being in ratio to the total mortality as 1 to 366, and to the entire population as 1 to 13,000. A ratio that has been found to vary little from year to year. The number found drowned was 40, two-thirds being probably suicides. Deaths from unknown causes amounted to 42; some of these no doubt being suicides.

The known suicides occurred—7 cases during each of the months of August, January, and June; 6 cases during each of the months of November, March, and May; 5 cases during each of the months of September and December; 4 cases during July; 3 cases during each of the months of February and April; and 2 cases during October.

As in 1859, the greatest number of suicides were among those in middle life, between 30 and 40 years of age. The youngest case was in an individual between 10 and 15 years old; the oldest between 60 and 70.

One-fourth of the cases were in those of native birth; one-fourth were Germans; another fourth, Irish; and the remainder of different European nations.

During the two years in 76 cases only is the mode of suicide reported, namely, in 36 cases by poisoning; by hanging in 20 cases; by shooting in 12 cases; by cutting throat in 5 cases; by drowning in 3 cases.

For poisoning the most common agents employed were laudanum and white oxide of arsenic; after these, oxalic acid, stramonium, Scheele's green, and cyanide of potassium. The suicides by shooting were mostly males; by hanging and laudanum were the most common forms of suicide among females.

On *bleeding in cerebral disease* is the subject of the next paper. It is by Dr. S. OAKLEY VANDERPOEL, of Albany. The circumstances which, according to Dr. V., forbid a resort to the lancet in diseases of the brain are, when the patient,

comatose and paralyzed, is cold and collapsed; when the action of the heart is feeble and intermittent; when there is present a state of anæmia; when the patient is of a very advanced age; if there exist evidence of extensive disease of the arterial system; if there was good reason to believe that already a large amount of hemorrhage had taken place within the cranium.

If, however, he remarks, the patient is of vigorous frame of body, if the face be flushed, the attack recent, and the pulse strong and full; particularly if it follows the sudden suppression of some accustomed hemorrhage, moderate bleeding is beneficial. The amount to be determined by its effects on the heart's action: the object being not to draw away blood from the brain, but to diminish pressure on that organ by lessening the force with which the heart propels the blood through the carotid and vertebral arteries. If too profuse, it will increase the irritability of the heart, while at the same time the blood is impoverished, thus actually favouring the condition it is sought to remedy.

The views advanced by Dr. V. in the paper before us appear to be, in the main, perfectly sound. His therapeutical conclusions do not strike us as differing very materially from those now generally recognized by our best practitioners.

The two next papers are on *Diphtheria*. The one by Dr. U. Potter, of Hallsville, the other by Dr. F. Jacobs, of Delaware, N. Y.

The first gentleman writes, that in his neighbourhood, within a circle of a mile or more in diameter, few persons, whether young or old, failed to exhibit more or less traces of the prevailing epidemic influence. The term diphtheria, however, was restricted to such as exhibited the characteristic exudation in the throat, whether in the form of isolated patches or of an extended membraniform coating.

According to Dr. Jacobs, the disease, as observed in Delaware Co., N. Y., was far more fatal on the higher grounds than in the lower valleys.

A case of *poisoning by corrosive sublimate* in which death ensued within forty hours, is reported by Dr. H. S. Downs, of New York. Upon examination of the body, the stomach was found to contain some four ounces of a grayish-black viscid fluid. Near the base of its anterior wall, at the cardiac extremity, was a patch nearly two-thirds of the size of the palm of the hand, of a bluish-black colour, resembling gangrene. A similar patch of the size of a quarter dollar, existed at the pyloric extremity, though less marked in disorganization. Five smaller patches, from two to four lines in diameter, intervened. There was one patch of reddened mucous membrane of the size of a silver dollar; the residue was of a grayish hue. The mucous coat was softened, but, excepting at the two larger points of black discoloration, was not broken. The intestines contained only a small quantity of a greenish pulp; the mucous coat seemed destroyed. Externally they had a slightly injected appearance, and felt gluey to the touch. The liver was fatty and congested. Nothing abnormal was detected in the other viscera of the abdomen, nor in those of the thorax. Brain not examined.

The next is the history, by Dr. J. G. Orton, of a case of *suspected poisoning*. Upon examination of the exhumed body of the deceased, forty-two days after death, extensive general decomposition of portions of the body having taken place, nothing of a very decided morbid character was detected, excepting that throughout, especially in its parenchymatous structure, the liver was studded with an immense number of minute crystals consisting for the most part of cholesterine, fat-globules, and hippuric acid.

Upon a chemical examination of the contents and tissues of the stomach and intestines, the former presented no trace of the presence of any mineral poison, while in every portion of the latter was detected the positive presence of corrosive sublimate, and, it was believed, in quantity sufficient to account for the remarkable state of preservation in which those tissues were found at the date of the *post-mortem* examination.

Two days previously to the death of the patient, a homœopath, it was proved, had administered to her a solution of corrosive sublimate every three hours. The strength of the solution was not ascertained.

The verdict of the jury was, that the deceased came to her death from causes to them unknown.

An interesting *case of suicide*, caused by the plunging of a shoemaker's knife into the right side of the throat, with such force as, after driving its point into the substance of the fifth cervical vertebra, to break the instrument off within an inch of its point, is related by Dr. T. C. FINNEL, of St. Vincent's Hospital.

This case is one which shows the great necessity of caution in deciding whether a wound inflicted in any given case is the result of a suicidal or homicidal intent. The position of the wound on the right side of the neck, its direction, and the violence evidently used, would, at first view, lead to the suspicion that the deceased had been murdered, but the circumstances of the case did not permit of any such conclusion.

A very interesting *case* of *compound, comminuted*, and *complicated fracture of the upper end of the tibia*, is related by Dr. ALDEN MARCH, of Albany. The fracture in this case was caused by the bursting of the driving-wheel of a threshing-machine, and besides its compound and comminuted character, it was complicated with an injury of the anterior tibial artery giving rise to considerable hemorrhage at the time of the accident, and at intervals subsequently, and the injection of a large mass of blood beneath the integuments, causing a circumscribed swelling some four inches below the knee, on the outside of the leg. Amputation was performed on the sixty-third day after the occurrence of the fracture, and was followed on the ninth day afterwards by death.

The case and its result bring up the question of the proper treatment of compound fractures; the rules in relation to which are very concisely summed up by Dr. M. Considering all the features of the case in question, he comes to the conclusion, that in similar ones, and under similar circumstances, provided a clear diagnosis can be made out, early amputation is the correct practice—the one which affords the only chance of saving the patient's life.

The next paper, by Dr. J. C. HUTCHINSON, of Long Island College Hospital, is the history of a case of *fracture of the spine*, caused by a fall, in which exsection of portions of the eighth, ninth, and tenth dorsal vertebræ was performed: with a tabular summary of twenty cases, in which the same operation was effected.

"A feature worthy of note in the above case," remarks Dr. H., "was the extent of the injury to the vertebræ and spinal cord, of which we were of necessity ignorant before the operation: and this suggests, in a very small compass, the leading arguments against it, viz: 1. We cannot, *in any case*, know the amount of injury which the vertebræ have sustained; whether there is a fracture of the processes, or of the bodies. 2. We are also ignorant of the extent of the mischief which has been inflicted on the cord, whether it be compressed, or lacerated, or divided; consequently, the operation may be inadmissible, on account of the state of the bones of the cord. If the case presented any features which would satisfy the surgeon that the fracture of the bone was limited to the arch, or that the cord was sound, or, if there was any reasonable probability that such was the case, I suppose the operation would be justifiable, but this state of the question decides it in the negative, except, perhaps, in such as Nos. 8, 9, and 10 of the table, where the patients survived the immediate effects of the injury, and were operated on several months or years subsequently."

The fourteenth paper is the account of an *instrument for the dilatation of stricture of urethra*, by Dr. BURGE, of King's County. The instrument is a metallic cylinder of the size of the largest bougie, containing within it a series of cylinders representing bougies of a gradually diminishing series. Carrying the instrument as far up into the urethra as can be done without difficulty, the next in size is to be protruded, and so on, gradually, from the larger to the smaller inclosed cylinders, each being always retained in place, as a guide to the next. The mucous membrane being thus kept sufficiently stretched, to prevent any folds of it forming to prevent the passage of the instrument, there is nothing to contend against except the stricture itself.

The next paper is on the best place for *amputation of the lower extremity, with reference to the adaptation of an artificial limb*. The author, Dr. DOUGLAS BLY, of Rochester, suggests "that in those cases in which the flexors of the foot

cannot be saved, the junction of the middle and lower third of the tibia," is the lowest part at which amputation of the leg can be performed, so as to give sufficient room for the adaptation of a good and graceful artificial substitute, with a well-constructed ankle-joint, leaving a stump of sufficient length to be serviceable to the patient. To this point, the surgeon, in amputating, should keep as near as circumstances will permit. If the entire leg, below the knee, has to be sacrificed, the amputation Dr. B. recommends to be performed just above the condyles of the femur, or as near to this part as possible, but never through the knee-joint, the size of the condyles interfering with the construction of, while it renders clumsy the artificial knee-joint.

The 16th paper is on *the treatment of fractures of long bones by simple extension without splints.* It is by Dr. J. S. SWINBURNE, of Albany. The plan proposed by Dr. S. is to place the patient in bed, and then pass a broad, well padded belt beneath the perineum and over the groin on the same side with the injured limb, in such a manner that the line of counter-extension made by it shall be through the long axis of the femur. This perineal belt is secured to the head of the bedstead. Extension is obtained by the application of adhesive strips, from half an inch in width, and sufficiently long to be applied along the outside of the leg, descend spirally, and protrude so as to form a strong loop beneath the sole of the foot, and then ascend upon the inside of the leg. These may be secured by strips or a roller bandage encircling the limb. A strong cord is to be passed through the loop of adhesive strips and secured to the foot of the bedstead. The advantages of this plan of treating fractures of the lower extremities are that it is far more comfortable to the patient, and allows him greater freedom of motion without endangering a displacement of the fractured bone. It enables the seat of fracture to be always readily examined, and in cases of compound fracture, gives free access to the wound of the muscles and integuments. It enables the length of the limb to be ascertained whenever desired, and the amount of extension required to be regulated with comparatively little trouble. It does not interfere with the circulation in the limb, and obviates the occurrence of sloughing at the heel or malleolus. It prevents agglutination of the muscles to the bone or to each other, and the consequent stiffening of the limb, and leaves the muscles in a condition for locomotion so soon as this is admissible. There is no difficulty with this plan of maintaining reduction and apposition of the fractured ends of bone; the chances of distortion or shortening being diminished; no case of non-union of the femur has occurred under its use. To prevent eversion or inversion of the foot, a bag of sand or bran may be applied on either side of the foot, or a strip of plaster or cloth made use of.

By the above method forty cases have been treated; twelve hospital, twenty-eight private. Of the latter, twelve were intracapsular. Fifteen cases of fracture of the femur treated by simple extension compare very favourably with those treated with splints.

The application of this plan of treatment to other fractures appears to have been found equally advantageous and successful. For the evidence in support of this, and for the modifications necessary in the character and application of the counter-extending and extending means according to the nature and seat of the fracture, we must refer to the paper of Dr. S., which will afford ample compensation for the time demanded for its careful study.

A case is related by Dr. T. GAILLARD THOMAS, of *recovery from suspended animation after chloroform*, by the use of Dr. Marshall Hall's ready method, after the lapse of twenty minutes from the period of the complete suspension of animation. After the action of the heart had recommenced, the galvanic battery was employed. But Dr. T. is convinced that the recovery was entirely due to Marshall Hall's method, the other means having only aided this in the accomplishment of the result.

Dr. A. VAN DYCK, of Oswego, relates a rather singular case of complete *inversion of the uterus.* The process of inversion was undoubtedly commenced at the delivery of the placenta by an unskilful midwife: undue force being exerted upon the funis to drag away the after-birth before active contraction of the uterus had commenced. The partial inversion then produced, gradually

progressed, until, on the eighth day after confinement, a little straining at stool rendered the inversion complete. The suffering of the patient abated after the third day. This Dr. V. thinks can only be accounted for on the supposition, that, at the period named, the uterus had so far descended into the vagina as to cause a remission in the previous spasmodic contractions of the organ. The reduction of the uterus in this case was effected readily by the usual manipulations. The patient suffered little or no inconvenience subsequently.

In the next paper is given, by Dr. C. V. BARNETT, a case of *rupture of the womb complicated with strangulated umbilical hernia* in a state of gangrene. The evidence of ruptured uterus was undoubted. To give the infant a chance for life, gastrotomy was instituted immediately upon the death of the mother. On removing the covering from the abdomen, an enormous umbilical hernia was discovered, its investments being absolutely black in consequence of the gangrenous condition of its contents. Upon opening the abdominal cavity the infant was found, all but the head and arms being without the uterus, in contact with the posterior surfaces of the abdomen. It was dead. The rent in the uterus was strictly longitudinal. In other respects the organ was healthy. The placenta was still adherent; no material hemorrhage had occurred.

The twentieth paper gives the history of a very interesting case of *polypus uteri*. It is by Dr. G. A. DAYTON, of Mexico, N. Y.

The next paper is on *rupture of the uterus*, with an account of three cases; by G. J. FISHER, of Sing-Sing. The cases described by Dr. Fisher occurred all in strong, healthy, respectable Irish women. The children were all males of large size, weighing from ten to twelve pounds. The presentation was natural in all. No premonitory symptoms gave warning of the approaching accident. The laceration occurred on the right side, involving in all the cases both uterus and vagina. In all the placenta had escaped into the peritoneal cavity, and the uterus was firmly contracted. No great positive hemorrhage occurred. One case was unattended by nausea or vomiting, or by recession of the child's head. In all, the outlines of the child could be traced with remarkable distinctness through the walls of the abdomen, serving as the most distinctive and characteristic symptoms.

In two of the cases delivery was effected by the forceps, and in one by craniotomy. All three cases terminated fatally to both mother and child.

From the result of these cases, taken in conjunction with the statistics of Dr. Trask, Dr. F. has come to the determination, should he be so unfortunate as to witness other cases of rupture of uterus, to resort without delay to the abdominal section, unless delivery could be otherwise accomplished promptly and without the slightest difficulty. He believes that in nearly every case it would increase the chances of recovery, unless, perhaps, where the vital powers are in a state of extreme prostration.

An interesting case of *sub-peritoneo-pelvic pregnancy* is related by Dr. JULIUS A. SKILTON, of Troy. After death, in this case, a ruptured tumour containing a fœtus, cord, and placenta, was found lying between the folds of the peritoneum and external to its cavity, beyond and below the ovary, and not in the Fallopian tube. The placenta was attached to the posterior fold of the peritoneum, and this being a serous membrane, its bloodvessels could not conjoin readily with those of the placenta, and, as a consequence, the growth of the fœtus was nearly suppressed. Absorption of that portion of the peritoneum to which the placenta was attached, resulted in rupture and fatal hemorrhage. A caducous membrane was found protruding from the os uteri. The fimbriated extremity of the right Fallopian tube was transformed into a sac which contained a small quantity of sanious fluid.

A *case* is reported in which the patient, a young married female between 22 and 23 years of age, was delivered consecutively *of twins, the second pair within one year and five days after the first.* The first pair of twins died within a few days of each other, when four weeks old. Soon after her first menstruation, fourteen months subsequent to the birth of the second pair of twins, the patient was attacked with intense peritoneal inflammation, and died on the ninth day of the disease. The case is related by Dr. Charles Burrows, of Clinton.

The twenty-fourth paper is the history, by Dr. R. J. HEMSTREET, of an *encysted*

tumour of pelvic origin. The case was an extremely obscure one. The presence of the tumour was not positively made out until after the death of the patient. She had complained for several years of a series of symptoms all pointing to some difficulty, probably the presence of a tumour, in the pelvis. Ascites was finally developed, for which she was tapped and six quarts of water drawn off, soon after which she died.

On opening the abdomen after death, the peritoneum was found thickened, an empty cyst was discovered from which the fluid had been removed by tapping; above this was another empty cyst, from which probably had escaped the fluid discharged through the puncture made by the trocar on the day she died. Occupying the hypogastric and pelvic regions was a tumour, originating low down in the pelvis, to the sides of which it was firmly attached. The apex of the tumour pressed forward the posterior walls of the vagina. The condition of the uterus was nearly healthy, excepting that it was thrown by the tumour upwards and outwards above the pubis. The urinary bladder was so compressed that its cavity was nearly destroyed. The tumour was composed of a congeries of cysts, from the size of a large pea to that of a goose egg, filled with a gelatinous fluid. On opening one of the cysts, it was found to inclose another smaller one, while it was probable this latter contained another still smaller.

A very remarkable *case of suppression of urine* is related by A. G. Purdy, of Madison County. The physicians, nurses, parents, and neighbours of the patient, a maiden lady, 36 years of age, all concur in the statement that, for nearly six years, the patient had laboured under complete suppression of urine. Previously to the complete suppression, the patient was obliged to use a catheter, at first daily, then at longer and longer intervals, at last of a month or two. The urine was described as dark and thick like the sediment of coffee. The face of the patient, originally fair, is now black, with the exception of the upper lip, and from the corners of the mouth downwards. The left arm and leg are also black. The discoloured skin appears as if covered with a thin coating of dry black blood. The covering of her face is not continuous but fissured, giving the resemblance of scales which touch without overlapping, and having considerable lustre. The change of colour commenced on the left hand, about the time the use of the catheter was discontinued. Occasionally, small quantities of blood ooze through the skin. A former physician of the patient states that he has seen her expectorate, during a paroxysm of coughing, a substance strongly resembling charcoal. Her parents say that for eight weeks she had not had an evacuation from her bowels.

The *report of the Committee on Medical Education* follows next in order. The report is replete with sound practical views. It condemns the proposition so earnestly maintained of the importance of a preliminary degree, licensing to practise, but subordinate to that of doctor of medicine. As the most essential means for elevating the standard of excellence in the candidates for admission into the medical profession, the report insists upon the necessity: 1st. Of a more thorough preliminary education before entering upon the studies of the profession. 2d. A longer and more thorough, by which is not meant merely a more extended, course of instruction in our medical schools; and, 3d. A more thorough and severe ordeal for the final examination to admit to the doctorate.

The next report is that of a special committee appointed to devise some *means of controlling the use of adulterated and inefficient medicines.* The plan suggested is for physicians to make themselves practically familiar with the physical and chemical tests of genuineness in the articles of the materia medica and pharmacopœia prescribed by them, and to make the cost of medicines, simple or compounded, always subordinate to quality.

A *report* follows, *from the delegation of* the New York State Medical Society *to* the fourth decennial *National Convention for revising the United States Pharmacopœia,* and then a report on *medical topography and systematic drainage.* The latter, with its several appendices, is a paper replete with interest, although its general details and conclusions all apply primarily to the geographical and topographical aspect of the State of New York. Still the leading practical truths embraced in it may be made available in the pursuit of a similar series of inquiries in respect to the medical topography and systematic drainage

of any other State. We cannot, however, afford the space a proper analysis of the report would demand.

A short *report* succeeds *from the committee on medical and surgical statistics.* It is, if we understand it, an explanation of the delay which has occurred in the presentation of the statistics for the past year deducible from the registrations of the practice of the individual physicians of the State.

The next report embraces a memorial relative to *the medical profession and its legal protection;* following which, are short *biographical* notices of recently deceased members of the Society; *A system of medical ethics* adopted by the State Medical Society of New York; the *Code of the American Medical Association;* and an Abstract of *the proceedings of the State Society,* at its fifty-fourth annual session; *list of members,* etc.

Preceding the list of members is an account, by Dr. J. MARION SIMS, of the city of New York, of an *amputation of the cervix uteri,* which was received too late to allow of its insertion elsewhere. The operation proposed by Dr. S. is to cut off by means of a scissors as much of the cervix uteri as is deemed advisable, then, by passing four sutures of silver wire through the anterior and posterior borders of the wound thus made and drawing them tight, to bring these borders or wounded edges into apposition in a straight line across the middle of the stump, so as to cover it completely, but leaving a small central opening for the os, just over the outlet of the cervical canal. The advantages of this plan are said to be the facility and rapidity with which the parts heal by the first intention, avoiding the danger of pyæmia, of degeneration of tissue, or of the stump contracting as it heals. In the three cases in which the operation has been performed by Dr. S. there was no hemorrhage requiring a ligature: after the operation menstruation was easy, though in each case the os was smaller than natural and required to be enlarged by slight incisions.

2. The session of the Ohio State Medical Society was opened by an address from the President, Dr. L. FIRESTONE, of Wooster; the subject of which is the office of the physician, together with the leading mission to be accomplished by our State and national medical associations. It is a sensible production, but presents no points beyond those which constitute the usual staple of similar addresses.

Following the address of Dr. Firestone is the report of the *Committee on Obstetrics,* drawn up by Dr. M. B. WRIGHT, of Cincinnati.

The report is an able one, while its contents are of a somewhat miscellaneous character. A case is related to prove that the presence of the hymen is no barrier to impregnation. Other instances are recited with the design of showing that sexual intercourse may take place while the female is asleep, or in an unconscious state—a circumstance which might become of some importance in a medico-legal point of view if it could be proved. A few cursory remarks in denunciation of the fearfully increasing practice of criminal abortion are made. Cases are related to show that abortion is a more frequent consequence of mental impressions than is generally supposed. Connected with this subject, facts are also adduced leading to the conclusion that the embryo may lose its vitality, while the placenta continues to increase considerably in size, and remain an indefinite period within the uterus, with a destruction of its natural functions. This fact, if established, must necessarily modify somewhat our treatment of threatened abortion. In the cases related in the report before us, the signs of pregnancy disappeared after having become prominent, and it was found, after the ovum was expelled, that its death must have occurred previously to the disappearance of those signs. The query then presents itself, whether it would not be judicious in those cases in which the distinct signs of pregnancy had disappeared to avoid the use of means calculated to prevent uterine action, and even to resort to such safe measures as are within our reach to facilitate the expulsion of the blighted ovum. The retention of a blighted ovum may not, it is true, be absolutely dangerous; it is, however, an inconvenience which the patient should not be permitted to incur.

The subject next touched upon in the report is the delivery of the placenta—the well being of the uterus demanding that it should, if possible, be thrown off

by the tonic contractions of the organ, to secure which should be the object of the practitioner as soon as the child is fully delivered and properly attended to.

Some remarks are made in reference to the extreme mobility of the uterus in its normal condition, in reference to the pathology of prolapsus and ante or retro-version. The morbid feelings which are usually described as being the result of prolapsus, Dr. W. is of opinion, are not, in fact, caused by mere change of position in the uterus, but arise from congestion or inflammation with morbid sensitiveness of the neck or body of the organ, whatever may be the position of the latter. Consequently, it is not to measures adapted to replace and retain in its position the dislocated uterus, our attention should be directed, but to correct the morbid condition, the congestion, inflammation, and abnormal sensitiveness of the organ, or perhaps, in some cases, of the vagina, bladder, or urethra.

"If careful and extended observation in future should be made, it will be found, we think," remarks Dr. W., "that the uterus did not duly contract after labour, and that this was mainly the cause of trouble. If this be true, the removal of the placenta before the uterus has made several ineffectual efforts to detach and expel it, is unsafe practice, and all the usual means should be carefully employed to secure a permanent tonic contraction of the uterus." "Upon examining cases, in which not only the general symptoms of prolapsus were present, but a soft and tumid uterus, we have applied tincture of iodine two or three times a week to the neck of the organ, and as it became condensed and less in volume, all symptoms of prolapsus have vanished."

Some remarks are made on the subjects of ruptured uterus, and the Cæsarean section. The latter operation, instead of turning, is advocated by Dr. W. in cases of rupture of the uterus, with escape of the fœtus into the abdominal cavity. Even in cases of rupture of the uterus with impacted head of the fœtus, where craniotomy is necessary to effect delivery, he believes that the Cæsarean section will be warranted in most instances, and afford a reasonable chance of recovery to the mother, when without, she must inevitably perish.

The report closes with a defence of the author's mode of effecting cephalic version from the adverse criticism of Professor Miller, in his recent work on obstetrics, showing its feasibility, the end to be effected by it, and the cases to which it is adapted.

The next report is on *cannabis indica* as a therapeutic agent, by Dr. R. R. M'Meens, of Sandusky. The diseases in which it has been recommended by different authorities, European as well as American, are tetanus, neuralgia, uterine hemorrhage; to increase uterine action in cases of labour, where ecbolic remedies are indicated: dysmenorrhœa; some forms of leucorrhœa; puerperal convulsions; chorea, delirium tremens, mania, shaking palsy, hooping and other spasmodic coughs; infantile convulsions, and the purely nervous affections generally. A number of cases are collected from various sources to show its efficacy in acute rheumatism, gastrodynia, spasmodic asthma, gonorrhœa, hysterical insanity, laryngismus stridulus, nervous rheumatism, chronic bronchitis, tetanus, etc. etc.

The report embodies a very valuable body of facts bearing upon the therapeutic uses of cannabis indica, and which establish its character as a valuable calmant, hypnotic, and anodyne in a very large circle of diseases. We must look, however, to future more extended and varied observations to determine the precise character of the cases, and the period of each in which its beneficial operation is the most certainly to be obtained, and the doses and combinations in which, under particular circumstances, it is best administered—with the conditions, if there be any, which contra-indicate its use.

The report on *Medical Literature*, by Dr. E. B. Stevens, of Cincinnati, presents a very general though sufficiently fair notice of the more recent additions to the medical literature of our country, and of the character and extent of its current professional periodical literature. The latter constitutes, confessedly, the especial instructor of the great body of physicians throughout our country—it being the medium through which the onward movement of the different departments of the healing art are first and most extensively made known to them. Its importance to the working members of the profession, and the influence which it is

adapted to exercise upon medical opinion and practice, press it strongly upon the attention of all who have the interests of the profession at heart, and demand that it should be constantly subjected to a cautious supervisorship on the part of our State and national associations.

The next paper in the Transactions is on the *effects of chloroform* upon the intellectual processes, being an inquiry concerning the credibility of testimony relating to transactions of a mind partly conscious. After a very extended inquiry into the capacity of the mind, when rendered partly unconscious either by a condition of imperfect sleep, or by any other cause, to recognize and distinguish the transactions which actually occur during that condition, so that it may bear accurate testimony in respect to them, from a metaphysical and physiological point of view, Dr. WRIGHT proceeds to examine into the condition of the intellect during certain stages of anæsthesia, produced by chloroform. The entire paper is marked by a high degree of interest. The inquiry to which it is devoted is conducted with great ability, and the premises and deductions of the author bear, with few exceptions, the unquestionable impress of truth. His general conclusions, for which alone we have room, are as follows; they appear to us to be in accordance with all the facts bearing directly upon the subject, which have been collected up to the present time.

"1st. That the will is always active while there is mental consciousness.

"2d. That the will cannot be directly impressed by another will, but that the judgment may be misled, and the will, though free, may act upon false conceptions, in a manner different from what it would do if the mind and senses were perfectly active.

"3d. There may be venereal connection with a female while she is conscious and unwilling; but there may be no venereal connection with a female, while she may honestly believe there has been, from her having been under the delusion of organic sensibilities, occasioned by the peculiar action of chloroform upon her nervous sensibilities.

"4th. It is impossible for a woman very often to decide whether actual connection has been had or not.

"5th. The evidence of a person respecting transactions that occur to the mind while partly conscious, is always liable to the most monstrous fallacies, and it should not be received as sufficient proof of any fact.

"6th. Evidence of females respecting rape upon themselves, while unconscious from chloroform, is particularly liable to suspicion.

"7th. Evidence of females respecting rape under such circumstances, should be subjected to all the rules and exceptions of circumstantial evidence, and should be fully corroborated by other circumstances."

A report of *the Committee on Obituaries* is next in order. It is drawn up by Dr. C. P. LANGDON, of Westerville, and comprises a short biographical sketch of Dr. H. A. Ackley, a recently departed physician of Ohio.

The report of the *Committee on Diseases of the Eye*, by Dr. A. METZ, of Massillon, after a short consideration of the condition of ophthalmic surgery in the United States, proceeds to give a sketch, and a very good one, of the symptomatology and treatment of purulent ophthalmia, and one of the most common terminations of the disease in its chronic form, granular conjunctiva. The report sets forth in a very concise manner the present state of our knowledge on the subject, but without the addition of any new observations in illustration or correction of received views, and modes of treatment.

The consideration of the deep-seated diseases of the eye commences with a notice of the ophthalmoscope and its uses. The diseases briefly noticed are cataract, and the operations for its removal; the morbid conditions of the iris and their surgical management; the nature and treatment of glaucoma. A very good *resumé* of recent observations, and teaching and practices, in respect to these ophthalmic diseases, is given by Dr. Metz, but nothing new.

A report is made by Dr. W. H. REEVES, of three cases of inflammatory disease of the neck of the uterus, treated with the local contact of nitrate of silver, by the reporter.

The next report is on the *condition of the medical societies of Ohio*, with a reply to a preamble and resolutions of the Toledo Medical Association, asking

for the adoption of measures adapted to "procure legal protection to the medical profession;" by Dr. G. F. MITCHELL, of Mansfield.

The Committee admit the necessity of legal protection, to secure to the people of the State the services of well informed and thoroughly educated medical men, and to prevent the injury to individuals and to society which results from the encroachment upon the profession, properly and legally constituted, of uninformed and incompetent individuals; the Committee are, at the same time, of opinion that it is impossible to attain this important end, at present, through legislative enactment, and that the remedy for the evils under which the profession labours is within the reach of the profession itself.

It is very certain that from our legislatures the medical profession can look for little aid: it is very much to be feared, that the inefficient and half way legal protection it would be inclined to mete out to it, and the almost insuperable difficulties that would be thrown in the way of its successful execution, would rather become, in the end, a premium for charlatanry, than a remedy for evils which now exist in respect to the proper practice of medicine in our midst.

The concluding report is on *insanity*. It is by Dr. R. GUNDRY, assistant physician to the Southern Ohio Lunatic Asylum, at Dayton. It is a very ably drawn-up paper, and furnishes a truthful summary of the more prominent questions connected with the etiology and management of that disturbed condition of the intellectual, moral, or instinctive faculties of the mind which constitutes insanity. From an examination of the most reliable statistics, Dr. G. indicates the ratio of insanity to the general population in various cities and countries, showing that the disease is most rife in the principal seats of civilization, and increases constantly with the progress of civilization. Propositions, the truth of which will admit, we think, of some dispute.

His investigation is next directed to the influence of seasons and climate on the production of insanity. The seasons range in respect to their efficiency in the production of the disease as follows; first summer, then spring, then autumn, and lastly winter.

In respect to the period of life at which insanity is most prevalent; from a great variety of statistics, Dr. G. concludes, that the greatest *absolute* number of cases occur between 20 and 30 years of age, while by comparing the occurring cases of insanity with the proportion existing in the community at the same ages a slightly increased proportion will accrue between the ages of 30 and 40—or that mankind at that age are brought into contact with more causes of insanity than at any other. The ages when the fewest cases of insanity occur are under 20 and over 60 years.

In regard to the influence of the several civic conditions, the statistics of insanity furnish insufficient data for the establishment of any very positive conclusions.

The next department of the report is devoted to a general consideration of the Causes of Insanity. From the reports of seventeen hospitals in the United States, Dr. G. has carefully classified the causes there assigned for the production of insanity in 22,815 cases—from which are excluded such in which "hereditary transmission" is assigned as the sole cause. Of the foregoing cases, he finds that rather more than four-sevenths, 44.72 per cent., are ascribed to *physical* causes, and something less than three-sevenths, 32.82 per cent., to *moral* causes, while in some 22.44 per cent. of the cases the cause was unascertained. Among the males over three-fifths, 47.09 per cent., of the cases in which the cause was ascertained, are referred to *physical* causes, and less than two-fifths, 29.05 per cent., to *moral* causes; while among the females, three-fifths, 48.62 per cent., of the cases are ascribed to *physical*, and two-fifths, 31.88 per cent., to *moral* causes; in 19.47 per cent. the cause was unascertained.

We are unable to follow Dr. G. in his remarks on the special causes included in each of the two general classes of physical and moral.

The report concludes with a general view of the curability of insanity.

3. The subjects of the annual address delivered before the sixty-ninth annual convention of the Connecticut Medical Society, by the president, Dr. ASHBEL WOODWARD, are Life, its characteristics as exhibited in the vital organism of man; the causes by which the degeneracy of that organism from its assumed

normal standard has been brought about; and the possibility and means of its renovation. It is a well conceived and ably written address, that may be read with profit: it presents for our notice, however, no very striking or novel propositions. The address is followed by an annual dissertation, having for its subject *hereditary predisposition;* it was read by Dr. JOHN B. LEWIS, of Rockville. The views of Dr. L. in reference to hereditary predisposition to disease are characterized by sound common sense, and are in conformity with the best authenticated facts in reference to the etiology of those classes of disease which are known to prevail in particular families, generation after generation. The means by which this hereditary tendency may be exterminated presents a subject for the investigation of medical men of vast importance; in it is involved the successful prophylaxis of some of the most serious of the maladies to which the human subject is liable—maladies, more than one of which have heretofore set at naught every plan of treatment that has been devised for the arrest of its fatal career.

We have next an interesting sanitary report from Hartford County, drawn up by Dr. L. S. WILCOX. By it we learn that during the year 1860 the whole number of deaths in the county was 1,530—males, 769, females, 750; sex not given, 11. Of these deaths 304 occurred in infants under 1 year of age; in those over 1 and under 5 years, 296; in children between 5 and 10, 96; in individuals from 10 to 20, 80; from 20 to 30, 139; from 30 to 40, 113; from 40 to 50, 87; from 50 to 60, 112; from 60 to 70, 98; from 70 to 80, 109; from 80 to 90, 57; from 90 to 100, 12. In 27 cases the age is not given.

The deaths by classification were—from zymotic diseases, 431; diseases of the respiratory organs, 346; of the nervous organs, 202; of the digestive, 66; circulatory, 33; generative, 16; urinary, 10; locomotive, 9; integumentary, 1; diseases of uncertain seat, 147; old age, 57; violence, 81; stillborn, 40; unknown, 91.

Percentage of deaths to the entire population of the county, 1.69.

In 1860 the deaths exceeded those of 1859 by 199. The excess was chiefly in the zymotic diseases; those of uncertain seat, and of the nervous and respiratory organs. The excess in zymotic diseases is 106, of those of the respiratory organs, 46. This large increase of deaths from these classes of diseases was probably due, it is stated, 1st to the unusually damp meteorological condition of the last year, the towns of greater mortality being those especially exposed, both in respect to soil and situation, to moisture and high winds; 2d. The prevalence of an epidemic influence in some parts of the county, manifesting itself especially in the occurrence of diphtheria. The number of deaths by the latter disease was 74; they nearly all occurred along the course and in the vicinity of the Farmington River. The first notice of the disease in the mortuary bills of the county was in 1859.

Some circumstances are referred to that would seem to indicate a low average of vitality and viability in the female as compared with the male, during the period of life from 5 to 45 years.

"The average mortality of females under 5 years stands for the past five years in ratio to that of males, at 100 to 116.8; but from this age on to 45, every decennial period brings in a larger mortality for the female than for the male. This unexpected result—omitting the process by figures—is contrary to the accepted expression of extended mortuary reports, and is so far confirmatory of the indication already suggested.

"The ratio of births for the past five years stands at 100 for females to 114.04 for males. Now, most physiologists give as forces determining the sex of the new being, these two prominent ones, viz.: greater relative age, and greater relative vitality.

"Hofacker in Germany, by a rigid application of this rule in regard to age, found the ratio of births to stand at 100 females to 103.4 males, where the age of the father was from one to six years greater than that of the mother. Sadler obtained also nearly the same result in Britain. By comprehensive averages of the whole of Europe, the births were as 100 females to 106 males, where the preponderance of age on the side of the husband is undoubtedly greater than from one to six years. Now the assertion may be safely ventured that in this county the relative age of the husband does not preponderate more than from

one to six years, which, on the results already stated, would suggest an anticipation, in the births, of a ratio standing at 100 females to 114.04 males, forcing the conclusion upon us that if these data are reliable, the vitality of the mother here is very low compared with that of the European mother; and both results point to the inference already drawn, viz.: that in this county the relative vitality of the male from 5 to 45 is greater than that of the female. This result, of however doubtful derivation, chimes in with the painful apprehensions of anxious observers of public health."

The remainder of the Transactions consist of a number of biographical notices of recently deceased physicians of Connecticut, including those of Drs. William Tully and Wm. S. Pierson, the one born in 1785 and the other in 1787. The first of these gentlemen was confessedly among the most learned and scientific physicians of New England. The *Materia Medica* of Dr. Tully, though incomplete, is nevertheless a monument to his industry, learning, and ability; and, even although the reader may not be able to yield assent to all the theoretical and practical views inculcated by its author, he cannot refuse to acknowledge the genius and skill manifested by him in the preparation of this work, his profound knowledge of his subject, and the importance of his labours for its illustration and advancement.

Dr. Pierson was chiefly distinguished as an obstetrician; his knowledge, skill, and experience, united to his happy faculty of inspiring his patients with confidence in his ability to carry them safely through their confinements, rendered him justly celebrated in this department of medicine.

4. The first of the communications made to the Rhode Island Medical Society at its session of December, 1860, is an address *on the relations of the young physician to the profession and the public*, by Dr. H. G. Stickney, of Providence. The address is marked throughout by good sense. It sketches in bold outline the trials and temptations, the pleasures and rewards to be expected by the young practitioner of medicine during the first years of his professional career, with a side glance at the popular medical epidemics by which he will be encountered and his onward progress perhaps impeded.

The second communication is in the form of a letter *on some points of military surgery*, addressed to Professor Frank H. Hamilton, of Brooklyn, N. Y., by Dr. Usher Parsons, of Providence R. I. The points discussed in the letter are the importance of free ventilation to the well being and ultimate recovery of the wounded in battle, and the propriety of the delay of severe surgical operations until the system of the patients has entirely recovered from the shock of the injury.

In respect to amputation for tetanus, the experience of Dr. Parsons is decidedly against it. He has amputated twice without any benefit. He has seen no case of successful treatment when the disease has developed itself. For the prevention, after a battle, of traumatic tetanus, he considers it of great importance to graduate the covering or clothing of the wounded in accordance with the temperature of the air—hot days succeeded by cool nights having been found the most productive cause of tetanus.

The third communication, from Dr. W. Owen Brown, of Providence, is the history of *cases of phagedenic ulceration treated by the external application of chlorate of potassa*, and of *scrofulous ophthalmia treated by the local application of tincture of iodine*.

The cases treated by chlorate of potash were one of phagedenic ulceration, of a most unpromising character; and one of severe phagedenic vulvitis: both terminated favourably. In conjunction with the chlorate as a wash, there were given internally quinia, iron, and other tonics, together with a generous diet. It was only, however, after the free application of the chlorate, that a rapid arrest of the ulceration took place.

Three cases of scrofulous ophthalmia were successfully treated by applying the officinal tincture of iodine, with a camel's hair pencil, over the outer surface of the upper and lower eyelids, some three or four times at intervals of a few days.

The fourth communication is the history of a case of *tubercle of the lungs resulting in pneumothorax* and death, by Dr. T. K. Newhall, of North Scituate.

The next paper is an account, by Dr. J. H. ELDREDGE, of East Greenwich, of a case of *interrupted and renewed lactation*. A lady, one month after her delivery, in consequence of a severe attack of disease, was obliged to wean her infant; at the end of six weeks it was again applied to the breast, and in the course of a fortnight the secretion was fully restored, and furnished sufficient nourishment for the child during the ordinary period of suckling.

The concluding communication is on *the epizoöty lately prevailing among swine*, by Dr. E. M. SNOW, of Providence. This confessedly is a subject of deep importance, whether we take into consideration its bearing upon questions of a strictly pecuniary character affecting the interests of our farmers and pork-dealers; upon those appertaining to hygiene; or upon those connected with general pathology, arising out of the recognized relationship between epizoötic maladies and epidemic diseases affecting the human subject. The contribution made by Dr. Snow towards the elucidation of the nature of the epizoöt which, in many parts of our country, has prevailed so extensively among swine, is deeply interesting, especially his history of the results of numerous dissections of the diseased animals. Dr. S. believes it to be a general blood disease, of which the pleuro-pneumonia, the diarrhœa, the intestinal ulcerations, the purpuric spots, the ulcers about the mouth, feet, and other parts are the local effects.

Appended to Dr. S.'s paper is an account of a case of pleuro-pneumonia in the cow, by Dr. Collins. A disease which Dr. S. believes will be found to be identical with the epizoöt which forms the subject of his address. D. F. C.

ART. XVI.—*Report on the Influence of Alcoholic Drinks on the Development and Progress of Pulmonary Tuberculosis.* By N. S. DAVIS, M. D. (*Transactions of the American Medical Association*, vol. xiii., 1860, p. 565.)

PROF. DAVIS commences his report by stating that within the last eight years the idea has been circulated through the various periodicals of the country, that alcoholic beverages are capable of preventing the development of pulmonary tuberculosis, and of retarding its progress after it has been developed. He states that this idea not being in consonance with many facts that had previously come under his observation, he was led to commence a record of all the well-marked cases of tuberculosis which came under his observation, in relation to the use of alcoholic beverages. He has recorded two hundred and ten cases. "The leading object in recording all these cases was to ascertain just how far the subjects of them had been under the influence of alcoholic drinks for a period of not less than one year previous to any active or noticeable symptoms of the tuberculous disease." Analyzing his cases in pursuance of this object, he finds that of the 210 cases 68 had used some form of alcoholic beverage almost daily from one to twelve years previous to the active signs of tuberculosis; 91 had used these drinks occasionally; and 51 had wholly abstained from their use. Of the 68 persons who were habitual drinkers, only 15 were drunkards. Of the 91 who drank occasionally, some were accustomed at times to indulge to an excess, and others drank very sparingly and only on some social occasion.

The author concludes from these results: "It is clearly demonstrated that the use of alcoholic beverages, however uniform their administration, and however long continued, neither prevents the development of tubercular phthisis, nor retards the rapidity of its progress."

The subject is one of interest, and not without practical importance. It is not, however, to be confounded with another subject, viz., the value of alcohol as a remedial agent in phthisis. As the author remarks, "It by no means follows that if these beverages are shown to be useless as prophylactics, they are therefore equally useless as remedies." Still it must be admitted, if it be true that the habitual use of alcoholic drinks affords no protection against the development of phthisis, the fact is an argument of considerable weight against

the probability of their remedial value. It is desirable, then, to inquire whether the statistical results given in the report by Prof. Davis warrant the conclusions which he draws from them. We propose to raise this inquiry.

Let us see, in the first place, whether Prof. Davis has presented all the postulates involved in the problem which he undertakes to solve. To determine the influence of alcoholic drinks on the development of phthisis, it is not enough to ascertain that a certain proportion of patients affected with this disease has been addicted to the use of these drinks. No one will assert that the use of alcoholic drinks affords an infallible protection against the disease. The question is, whether they who habitually use these drinks are rendered thereby less liable to this disease than they would otherwise be. Now, it is ascertained that a certain proportion of a given number of patients affected with phthisis have been accustomed to use alcoholic drinks. This is one point in the problem. Another essential point is to ascertain whether this proportion falls below or exceeds the proportion of those who, in like manner, have used these drinks out of an equal number of non-tuberculous persons in similar conditions of life. It is plain that we must have some standard of comparison by which to judge whether the proportion of drinkers among patients affected with phthisis be so large as to show no antagonizing influence of alcoholic drinks, or so small as to lead to an opposite conclusion. Prof. Davis appears not to have considered the importance of this in assuming that the results of the analysis of his cases leads to the conclusion which he draws from them. In fact, the statistical results of his analysis constitute but a preliminary step to the inquiry concerning the influence of alcoholic drinks on the development of pulmonary tuberculosis. Having obtained these results, the next point to be settled is, whether they show among tuberculous patients a large or a small ratio of those who are addicted to the use of alcoholic drinks. How is this point to be settled? Plainly, the only way is to ascertain the ratio among patients not tuberculous, from the same classes of society of those who are equally addicted to the use of alcoholic drinks, and then institute a comparison of the results of the two analyses. Whether the results of Prof. Davis's analysis prove the influence of alcoholic drinks to be favourable to the development of phthisis, or otherwise, is thus, as we conceive, a problem to be settled by facts to which Prof. Davis makes no reference. In the absence of these facts, we have as much right to assume that the statistics given by Prof. Davis prove the prophylactic influence of alcoholic drinks, as he has to assume an opposite conclusion. Without assuming the former, we believe it will be found to be true.

The cases recorded by Prof. Davis occurred in hospital and private practice. He does not state the proportion of hospital cases, but we infer that they constitute the large majority, from the fact that, of the 210 cases, in all but sixty the patients were foreigners, and the author indeed gives, as an explanation of the large number of patients from Ireland (85), the fact of his being brought into connection with this class in the Mercy Hospital of Chicago. We will suppose all the 210 cases to have been hospital cases. In 51 of these 210 cases the patients had wholly abstained from the use of alcoholic drinks. This is certainly a large proportion of persons practising total abstinence to be found among the classes of patients received into a charity hospital. Would there be found 51 persons who had practised total abstinence for years among 210 hospital patients not affected with tuberculosis? But of the remaining 159 cases, in 91 the patients only used alcoholic drinks occasionally, some very sparingly and only on festive occasions. Now, it is absurd to suppose that an occasional indulgence, whether sparingly or in excess, is of any appreciable account in determining the influence of alcoholic drinks on the development of phthisis. These cases are to be excluded. So far as any influence is to be attributed to alcoholic drinks, the class of cases only is to be considered in which the persons "had used some form of alcoholic beverage almost daily from one to twelve years previous to the active signs of tuberculosis." To this class belong 68 of the 210 cases, or a fraction over 32 per cent. Now, is this a large or a small percentage? This question is to be answered after ascertaining the percentage of a similar class, as regards the use of alcoholic drinks, in 210 hospital cases not tuberculous. We are not in a situation, at the present moment, to appeal

to facts, but we shall be surprised if they do not show a larger percentage; if so, assuming that Prof. Davis's cases were mostly hospital cases, his statistics will clearly prove the reverse of the inference which he deduces from them. Taking the statistics as they stand, we may reason *for* a prophylactic influence of alcoholic drinks precisely as Prof. Davis does *against* such an influence, and, as we conceive, with greater force. Prof. Davis's argument is, so large a proportion as 68 of 210 tuberculous patients being habitual drinkers, it is proved that alcoholic drinks exert no influence to prevent phthisis. *Per contra*, it may be said, so large a proportion as 142 of 210 tuberculous patients not being habitual drinkers, it is proved that alcoholic drinks do exert a positive influence in preventing phthisis.

With regard to the value of alcohol as a remedy in pulmonary tuberculosis, Prof. Davis admits that an apparent improvement may be produced by it, but he says, "Truth compels me to say that I have never seen a case in which this apparent improvement was permanent." Inasmuch, however, as he states that the idea of alcoholic beverages preventing the development and retarding the progress of this disease when it was announced, was not in accordance with his views, and he began to collect cases in disproval of this idea as long ago as 1855, it is fair to presume that he has not employed alcohol in the treatment of this disease to much extent. We confess ourselves to be among those who have been led by clinical observation to attach no small value to alcoholic beverages in the treatment of phthisis. We are certainly able to cite a number of cases in which we have seen not merely a temporary, but a permanent improvement under their use, in conjunction with hygienic influences. But we reserve the consideration of this subject for some other occasion.

In concluding this notice, we would say a word or two with reference to statistical researches in medical investigations. Admitting their incalculable value, they are as likely to contribute to error as to truth, if it be not constantly borne in mind that the results of statistics lead to correct conclusions only by means of logical methods of reasoning. Facts in themselves accurate may be made subservient to error by regarding them from a limited point of view, or by false logic. It would not be difficult to find instances in which statistical facts have been adduced as the basis of certain conclusions, when a critical examination showed that such conclusions are not legitimate, if indeed they are not precisely the opposite of those which are to be legitimately deduced from the facts. Erroneous conclusions based on statistics are apt to remain long undiscovered, for most persons are not fond of looking into the statistical details. but accept the conclusions, presuming that they are logically deduced. These considerations afford a sufficient apology for our critical notice of the paper by Prof. Davis, if any apology be needed beyond the importance of the subject. If we have erred in our criticism, we shall be glad to be corrected.

With regard to the influence of alcoholic beverages in the prevention and treatment of phthisis, we insist that, as a purely medical subject, it be separated entirely from its incidental relations to other subjects. To speak more plainly, as a medical subject it is not to be mixed up with matters pertaining to temperance. In saying this, we mean no disrespect to or disparagement of the latter subject. Far from this, we hold the subject of temperance to be one of vast importance to the physical and moral welfare of mankind. All that we mean to say is, the subject of temperance is one thing, and the influence of alcohol on disease is another subject; and to secure a proper investigation of the latter, the two subjects should be kept distinct. Limiting our attention to pulmonary tuberculosis, when we have ascertained the nature and extent of the influence of alcohol in this disease, assuming that it does exert a favourable influence, then it becomes a highly important question, how far is the use of alcohol as a prophylactic or remedial agent in this disease objectionable on the score of favouring intemperance? For one, we have no disposition to ignore or to undervalue this question, but we claim that it should not be involved in the study of therapeutics so as to bias the mind in pursuing clinical researches; and, moreover, we claim, in behalf of those who deem it their duty to advise alcoholic beverages in the treatment of phthisis, exemption from the imputation of being unfriendly to the cause of temperance. A. F.

ART. XVII.—*Proceedings of the American Pharmaceutical Association, at the Ninth Annual Meeting, held in the city of New York, September, 1860, with the Constitution and Roll of Members.* Philadelphia: 1860.

THE Proceedings of this interesting and important Association, although not constituting so bulky a volume as that of the preceding year, are calculated to attract the attention not only of pharmaceutists but of physicians, who are equally concerned in the progress of pharmacological knowledge. From the statements made by members, it appears that the meeting of the Association, from which these Proceedings were derived, was as harmonious and earnest as on former occasions, and was even more numerously attended, contributing to the private gratification of the members as well as being productive of utility to themselves and to the public. We propose to present a summary of these Proceedings, with such remarks as may appear relevant to the subjects as they are presented to us.

The opening chapter is in the form of a prefatory note from the Executive Committee, which contains, among other things pertaining to their office as publishers, the recommendation to authors of papers to write in such a way as to facilitate the work of the editors, a circumstance which is apt to be overlooked by those not accustomed to write for the press. The two points most usually neglected, and strenuously insisted on, are the correct spelling of proper names, and legibility. A recommendation would also have been in place to write on but one side of a sheet, for which observance printers are most grateful, as it vastly facilitates their operations. In the report of the committee, which is the first paper of importance that follows, the committee has the melancholy duty to announce the death, during the year, of no less than seven prominent fellow members.

In the annual address, the President (Mr. Samuel M. Colcord, of Boston) speaks thus:—

"It gives me pleasure to congratulate you upon the very prosperous condition in which we commence the ninth year of our labours. Our rapidly increasing numbers, and the amount, variety, and quality of talent now at our disposal, and continually augmenting, gives promise of a bright and hopeful future, of a development commenced that shall place pharmacy with the foremost in the sanitary reforms of the day, performing an active part in removing that stigma from medicine produced by systematic overdosing—of which homœopathy and the prevailing medical infidelity is a counterpoise, and which, like the services of counter-irritants, having performed their uses, should be abandoned."

This would seem to be a happy hit, and will do well for a merely pharmaceutical mind, but it overlooks the fact of the progressive character of medicine itself, and a want of knowledge of the labours of the great medical minds who have lived within the last thirty years. Broussais' principles and practice did more towards breaking up the administration of large doses of medicinal agents than the introduction of infinitesimal medicine. True improvement in the art of prescribing has kept pace with the advances in pathological and therapeutical knowledge, aided by the presentation of better pharmaceutical preparations which the science of pharmacy has given to medical men. These afford the reason of improved and more satisfactory treatment of disease.

There are some other matters in this address, which we may casually mention. It appears that the subject of a charter has been agitated. With reference to this, the President states that "in fact the present sphere of our operations does not seem to require a charter." The difficulty, as in all similar associations, lies in the universality of the association. A local charter would make it a State institution, and thus break up the general and widely diffused interest that attaches to a society belonging to the whole country. We do not see, in the present condition of things, that, with all its advantages, an identity could be brought about with the British Pharmaceutical Association. Education, which is one of the objects thrown out as worthy of encouragement, in our humble opinion will be better encouraged, not by organizing "a learned faculty

on a uniform national basis," but by the systematic recommendation and patronage of the competent local schools, which have done such good work in placing pharmacy where it now stands. Reference is made to the *Pharmacopœia*, with the recommendation for its exclusive adoption and support. The sale of poisons and the Sunday trade are alluded to. The latter has been the subject of discussion for some time. The difficulty in closing the shops of dispensing apothecaries arises from the exigencies of the sick, and the corresponding demands for medicines on the part of prescribers. Sickness occurs on every day of the week; the physician in his visits of relief has no Sabbath day. He cannot crowd his work into a few early hours, and in carrying out his mission must have the assistance of his fellow labourer, the apothecary, in our large cities. If it be meritorious to lift an ox or an ass out of a pit on the Sabbath day, can it be desecrative to administer relief to God's image? This, however, does not apply to the general trade of the apothecary and druggist, which should be confined to the limits of other occupations. There is a common sense view of this matter which should be carried out in every establishment, and when so done will relieve both the master and apprentices from inordinate duties. The address throughout is marked for its soundness, deep interest in the science of pharmacy, and decision of opinion which characterizes its author.

Upon looking through the minutes, it will be perceived that many topics of importance occupied the attention of the association, and were fully discussed. Among others, the question of legislative interference in the sale of poisons, was fully entered upon. This is of the utmost consequence to the community, and we are glad to find that the whole subject has been referred to a committee to mature a report and present it to the next meeting. The Legislatures of several States have passed laws to regulate the sale of poisonous substances, but they are complained of as defective. The reason lies in their having been drawn up hastily, and without the full requisite information on the part of their framers. It is to be expected, then, that the enlightened action of this committee will hereafter result in the right and effective legislation by our rulers.

The first report of committees is that on the Progress of Pharmacy. It is divided into several parts, as—the condition and number of the schools and societies, necrology, pharmaceutical literature, the drug market, materia medica, pharmacy, inorganic chemistry, organic chemistry, toxicology, sale of poisons, &c. This report is eminently faithful, and must have cost its author, Mr. Edward Parrish, much labour and research. It may be pronounced an admirable summary, extremely valuable from its extent and fulness.

The next report is on Home Adulterations. At the commencement it is remarked that, "Your committee feel deeply the responsibility of their duty; they feel that much ought to be said and done which they dare not take the responsibility, as individuals, of saying and doing. They trust the time will come when State Legislatures, or the general government, will cause such penalties to be affixed to the selling of adulterated articles, that the business will cease to 'pay,' and that 'honesty' will be practised because it is the best policy. During the past year, your committee have had many articles of drugs, medicines, and food submitted to their examination, and they have felt more and more the necessity of some means being devised for checking this abominable practice, which, in the language of another, is undermining the very foundation of trade, viz., faith in commercial integrity." The reasons presented for practising adulteration are given as follows: *First*, for the purpose of making the substance more saleable, by improving its appearance by the addition of some body either innocuous or otherwise. *Second*, to depreciate the quality by adding some substance which will diminish the *real* without altering the apparent strength or general appearance. This is generally a very deadly fraud. *Third*, to depreciate the quality and 'extend' the quantity, by the addition of some simple substance, as water, or, if a solid body, as sand, gypsum, &c."

In this connection the use of lead pipe for the introduction of water is commented upon, and the remark is made that, "it is a question upon which scientific men have differed." The question resolves itself into one of pure science, depending upon the nature of the water thus carried. If water is charged with such salts as our Schuylkill water, the prevention of any evil consequences

results from their behaviour on the interior surface of the leaden pipes, protection is afforded by their presence, and reactions occur whereby a protective coating is formed. We have never known or heard of a case of lead disease traceable to the water of Philadelphia, but this is not so elsewhere, and the subject is therefore one of hygienic interest. As the report is from the pen of a Boston gentleman, (we presume,) whose name stands at the head of the committee, and from the citation of cases, the warning is well made for impression in all districts where danger may be lurking, and especially is this the case in the rural districts where the purest water is to be found, uncontaminated with calcareous or magnesian salines. The report also contains some interesting information on the subjects of milk, bread, and wine, opium, nitrate of silver, and chloroform.

The special reports are upon the following matters: Oleum æthereum, citric acid, sulphate of ammonia, emplastrum assafœtidæ, atropia from American grown belladonna, production of alcohol in the United States, aloin, paraffin, carrageen, production of conium seeds in the United States, guaiac, Smyrna opium, hops and lupulin, patent medicines, camphor, garlic, coca, and cacao. It is impossible for us to go into an analysis of the investigations that have been entered into on these items. It will be sufficient to intimate that they have been handled by the most intelligent and industrious pharmaceutists of the country, and are of decided importance as additions to our knowledge.

The volume of Proceedings closes with a number of volunteer reports and essays. The first consists of Remarks on Manufacturing Pharmacy, by Mr. Faber, of New York, which has some practical suggestions. The next is on Dice's Liverpool Hydrometer, by Dr. Wilson H. Pile, of Philadelphia. It is drawn up with his accustomed accuracy and painstaking. With reference to this gentleman of so much unpretending worth, it may be without impropriety stated that he has placed the medical as well as manufacturing community under deep obligations, by bringing the instruments for determining the gravity of fluids to a state of perfection and accuracy, before not attainable without great care, and dependence on a foreign source, with attendant expense. His gravimeters, as we know from our own experience, are the most accurate in the market.

An essay on hydrobromic acid and on bene culture will be found worth perusing. The former is from the pen of our friend John B. Maisch, of Philadelphia, who is doing good service to his adopted country by giving it the benefit of his German thorough, scientific, and analytical training. The sketch of the life of Cavendish is a good epitome of Wilson's life of the great chemist and philosopher, published by the Cavendish Society some years ago. The appendix contains a list of plants imported by the Patent Office, the Constitution of the American Pharmaceutical Association, and the roll of members.

The contents of this volume certainly exhibit the fact that the pharmaceutical profession are deeply in earnest in their endeavours to promote the advancement of their profession, and that the spirit of scientific research has been fully awakened in the United States. It is to be regretted, however, that the laudable efforts of which we have presented the manifestation, have received a check from the disturbed condition of the country, and that the preparations made for the meeting of this year have been held back for a more opportune occasion. The meeting of the Association was to have been held in St. Louis. A brighter day must come, and then scientific pursuits can, under auspicious circumstances, assume their wonted channels. J. C.

Art. XVIII.—*Transactions of the Obstetrical Society of London. Vol.* II. *For the year* 1860. *With a List of Officers, Fellows, etc.* London, 1861. 8vo. pp. 368.

The Obstetrical Society of London, the Transactions of the third year of whose existence are recorded in the volume before us, has proved, thus far, a most successful organization. Among its three hundred and fifty fellows are included a

large number of obstetricians, who by their talents, their industry, and their faithfulness, are fully qualified to carry out the important objects had in view in its organization; of busy practitioners, willing and able to cultivate fully their respective fields of observation, and with a zeal and method which cannot fail to develop principles and practical results calculated to enlarge, improve, and confirm the science and the art of midwifery.

The contents of the first volume of the Society's Transactions gave an earnest of the value of its future labours; an earnest which has been fully redeemed by the contents of the volume which now demands our attention.

This volume comprises, besides an anniversary address by the President of the Society, Dr. Edward Rigby, *thirty-four* articles. Of these twenty-one are histories of obstetrical cases of more or less importance; seven are devoted to an examination of questions of considerable interest in the department of practical midwifery; two describe cases of monstrosity in the fœtus; the remaining papers are devoted to a history of midwifery in the East; to an examination as to the power and act of propagation among the industrial classes of London, and to an account of a case of sloughing from reckless vaccination.

We shall endeavour to give as full an account of these articles as is in our power without being tedious.

The President of the Society, in his anniversary address, delivered January 4, 1859, makes mention of a movement on the part of the body having for its object a revision of the existing regulations of the several examining and licensing bodies of London as respects the medical education of obstetrical students, which regulations in their present state are pronounced to be "insufficient for insuring such an amount of proficiency as this important branch of the healing art not only deserves, but requires—the quantum of knowledge in midwifery being far inferior to what is demanded in the sister branches of medicine and surgery." The address contains, besides, short biographical sketches of three of the deceased fellows of the Society, Drs. Lever and Griffith, and Mr. Squibb. The first died on the 29th of December, 1858, the second June 23d, 1858, and the third in the month of January, 1858.

The first paper is the history of "*an intra-uterine fibro-plastic tumour*, removed by enucleation;" by Dr. J. Hall Davis. The patient was 51 years old; had menstruated regularly up to June, 1859. The flow was always large, but unattended with pain in the back or other suffering. For nine years had had much leucorrhœal discharge, sometimes purulent, and occasionally flooding. Had suffered for ten years from a gnawing sensation at the hypogastrium, and experiences much inconvenience in the pelvic region when riding in a carriage; and from frequent inability to void her urine has repeatedly to introduce a catheter. The urine is of a light amber colour, non-albuminous, and otherwise healthy in appearance. The patient was rather above the medium height, her habits were very active, and she spent much of her time in her garden. Had never suspected the existence of any uterine malady; had never undergone a vaginal examination. Palpation of the abdomen (September 29, 185?) showed the bladder to be so distended as to reach midway between the umbilicus and pubes. Between two and three pints of urine were drawn off. A hard circumscribed body was now detected extending symmetrically above the pubes, supposed to be the distended uterus; which diagnosis was confirmed by an examination per vaginam. By this it was found that the uterus was enlarged, and its neck obliterated, in consequence of the development of the organ for the accommodation of a growing tumour. The orifice of the uterus was open to the size of half a crown; within it was felt a roundish, smooth tumour, adherent to the inner surface of the uterus anteriorly, and on the right side down to the margin of the orifice; on the left side the adhesion extended to within an inch of the orifice, while posteriorly the tumour was entirely free up to the fundus of the uterus, to which it extended. A few days later an attack of flooding occurred, after recovery from which the patient went to her home in Devonshire. She returned to London about the 25th of October and placed herself under the care of Dr. Davis. Soon afterwards another flooding occurred, which, however, entirely ceased before the end of the month.

On the 1st of November, by introducing his finger through and within the

reflection of mucous membrane, passing from the cervix to the inferior surface of the tumour, Dr. Davis made the attempt to enucleate the latter from below upwards. Beginning in front, he detached the tumour up to a line beyond which he could proceed no further. Latterly he was able to proceed with the detachment somewhat higher up. Dr. D. believed that at least half if not two-thirds of the tumour was now detached. He desisted from any further attempt, fearing to do mischief in consequence of the firmness of the remaining attachment. There was very little hemorrhage during or after the operation. The uterus was washed out with warm water. The patient passed a good night, without pain.

Next day Dr. D. attempted to remove at once the detached portion of the tumour by the application of the écraseur, while the patient was under the influence of chloroform. Upon trial, however, it was found impossible to adjust the instrument. A whipcord ligature was, in consequence, carried around the tumour at the upper limit of its detached portion.

Things went on very well until the 5th of November, when symptoms occurred which though not in themselves of a particularly alarming character, were nevertheless sufficient to make Dr. D. anxious about the patient and desirous to get rid of the tumour as quickly as possible, from a fear of the occurrence of pyæmia from the absorption of putrescent matters produced by the dead portion of the tumour. The canula and ligature being removed, and the uterus and vagina well washed out with a solution of permanganate of potash, two drachms to a pint and a half of water, a pair of Museaux's forceps were guided upon the index finger in a direction to get a firm purchase upon the lower part of the tumour. By means of the forceps the tumour was gradually drawn down so that the part below the ligature, which was putrid, flaccid, and very compressible, passed into the vagina. Small portions broke away from time to time, upon which a firmer hold was taken higher up. Proceeding with the tractions and cutting off some portions, the putrid portion of the tumour, about two-thirds of the entire mass, was brought through the os externum, and a glimpse obtained of the parts above the ligature. During the operation firm pressure was applied above the pubes. The uterus, uninverted, was brought lower into the pelvis, and after the larger part of the tumour appeared externally, the organ had undergone some contraction of its muscular fibres and reduction of its bulk. By proceeding with the traction, getting continually a fresh hold higher up, and, finally, by a combination of traction with a twisting of the tumour close to its remaining adhesion, it was removed entire from its connection with the uterus. The operation lasted nearly half an hour. There was scarcely any hemorrhage. The uterus was now again washed out with the same solution as before, and some port wine diluted with water was given, as had been done during the operation. Opium by the mouth always disagreeing with the patient, none was given, but a poultice of linseed meal with the addition of laudanum was applied to the hypogastric and sacral regions, where some pain was complained of. Under a very simple treatment the case did very well.

At the end of a fortnight from the application of the ligature, Dr. D. writes, my patient was able to take a drive, and at the end of three weeks she was well enough to return home.

"The operation of the ligature on the tumour had the effect of facilitating my subsequent proceedings in its removal, and appeared, by weakening its adhesion, at first so firm as to resist all prudent efforts to break it down, to make the final separation a matter of no great difficulty.

"On examination the upper part of the tumour presented a perfectly fresh appearance; below this was the groove of ulceration formed by the ligature, beneath which was the putrescent portion."

The tumour was submitted for examination to Dr. Jenner.

"On microscopic examination, he found it to be composed of fibrous tissue, with a very large intermixture of fusiform nucleated cells, but no cells of the kind supposed to indicate a malignant character. It therefore belonged to the order of fibro-plastic tumours of Lebert. Its outer covering was composed of cellular tissue, with fine vessels. The lower part was covered by a reflection of mucous membrane. The mass required to be soaked in many waters before it

parted with its contained blood; hence it must be inferred that it was highly vascular, a point in which it differed from ordinary fibrous tumours."

We have next the account, with the drawing of "a large *congenital encephaloid tumour* not impeding delivery," by Mr. Spencer Wells. The tumour was of an ovoid form and considerable size, projecting downwards from the surface of the chin and left side of the face. The infant lived three weeks after birth.

The tumour, upon examination, proved to be an encysted mass of encephaloid matter, separated by dissepiments into imperfect cysts or areolar spaces, chiefly attached to the lower jaw and the left side of the larynx, and receiving large bloodvessels both from the carotid and axillary arteries. The cranium was quite perfect and the brain and dura mater normal.

The third paper is on the "*action of belladonna upon the mammary glands*," as a means of preventing and arresting mammary abscess, by Mr. Richard Marley. The writer has employed it in over forty cases, under circumstances most likely to produce mammary abscess, with a success which, he affirms, has enabled him heartily to recommend its early application. After pointing out the importance of the purity of the extract used, he lays down the following rules for its employment.

It is to be smeared on, not rubbed in. Friction is liable to injure the already irritable and inflammatory gland. The application should be made night and morning, or oftener, according to the urgency of the case and symptoms.

In summer the extract may be applied pure, or mixed with a small portion of glycerine. In the winter, when the extract is firm and unyielding, it requires to be well rubbed up with glycerine, in the proportion of two or three drachms of the latter to one ounce of the former. For fanciful ladies, a few drops of essence of amygdale may be added. The bosom should be kept supported by a broad handkerchief. The bowels are to be kept well open by salines. The diet should be spare. The patient should be warned that smarting will occur if any of the extract get in the eye, and that, if the infant be suckling at the well breast, care be taken none gets into its mouth. The use of the extract should be commenced with early, the moment there is the slightest indication that an abscess is about to occur. The extract should be applied over the entire surface of the affected breast, the areola included.

Dr. Marley remarks, that in "all large suppurating inflammations near the surface," the application of the extract of belladonna is attended with an almost immediate alleviation of the painful shooting and throbbing. The process of suppuration may go on, but the pain, frequently so distressing, is checked.

A description is given by Henry G. Times of a "*large fibrous tumour of the womb*," in a case which terminated in the death of the patient. The most interesting feature in this case was the fact that, notwithstanding the small amount of pain and constitutional disturbance experienced by the patient during her life, it was shown, upon examination after death, the tumour had grown from and *out* of the womb, and, owing to its excessive weight, had completely disconnected the cervix from the body of the uterus, as was proved by a probe, when passed through the os tincæ, entering a cul-de-sac, about an inch and a half deep.

A most interesting paper follows next, on "*Midwifery in the East*," by Dr. J. Jackson. The paper gives not only a succinct account of the practice of midwifery as it prevails in the East among both the European and native populations, but also of some of the diseases to which women and children are there liable. We had marked a number of passages for insertion in our present notice, but must content ourselves with quoting alone the following.

"In regard to the native woman, the mode of conducting the process of parturition is simple enough amongst the poorer class, and it is astonishing how quickly they recover from the effects of the labour, being able to go about their household work on the third day. With the higher class of native women, where caste prevails, the poor creature is considered an outcast at this particular time; and instead of having every tenderness and affection lavished upon her, as in this country, she is placed during the pangs of labour in the meanest hovel in the yard, as an unclean creature, or she is put into a matted shed in a lower verandah of the house, where, lying upon the floor, she is exposed to the heated fumes of wood and charcoal fires, which induce a degree of stupefaction and anæsthesia.

And thus, in their way, the Hindoos would seem to have discovered a plan which for ages has been in use before ether or chloroform, for a similar purpose, was found out. This process goes on during the whole period of labour. The temperature of the room is greatly raised, and it is filled with smoke; and were it not that there is free ventilation through the interstices of the matted walls and roof, the patient, as well as attendants, would run the risk of being poisoned.

"At the moment of delivery the woman is placed upon her back, her legs outstretched, and the midwife, seated on the ground in front, continually applies oil to the vagina and soft parts with her hand. At the time of protrusion of the head she places her feet at the nates and perineum, and bending over, with her hands she supports the vertex and steadies the entrance of the head into the world. At the time of delivery a firm cloth is tightened over the abdomen whilst the afterbirth is coming away. Stimulating warm powders, containing cloves, pepper, and ginger, are administered, and the room is kept warm for some few days. But she is considered impure for many days, according to the caste she has. A Brahminian woman is considered unclean for twenty days; other castes require twenty-eight days."

"It is rarely that the aid of the European obstetric is demanded for the native female. The latter, however, marry so young, and at times there is such a disproportion in size between the sexes, especially in the Mussulman population, that in such ill-matched unions there is delay in the delivery of the child, and the aid of the forceps may be required; but this is very rare."

"Occasionally the native midwives in India, as elsewhere, make use of violence, and I have known the head of the infant to be separated from the body, or the body placed in such position that the aid of the European surgeon has been required; such as when the arm has been firmly drawn out through the vagina. One case, from a trustworthy authority, was related to me, where the uterus was entirely inverted, and the midwife removed the whole organ, by dividing the parts with a knife. I have seen myself such cicatrization to have been caused as to close entirely the passage of the vagina, and thus prevent the escape of the catamenial discharge. This case was brought to my notice by the husband, who stated that his wife had been delivered, after a hard labour, nearly three years before. A crucial incision gave discharge to a treacle-like fluid, the retention of which had produced a distended state of the abdomen, that had been considered by the native practitioner to be the result of dropsy, and had been treated as such by him. I have seen closure of the vagina in a young girl giving rise to a somewhat similar condition."

We have next a "*case of Fallopian pregnancy*," terminating fatally, by Henry Grace, Esq. These cases are always interesting. They must be studied, however, in detail to derive from them data for the establishment of a correct diagnosis in cases in which extra-uterine fœtation is suspected. The fœtus in Mr. Grace's case was contained in an enlargement of the left Fallopian tube about its middle. It was about the size of a walnut, and had adhering to it a small clot of blood.

"On carefully incising this enlargement, a layer resembling the deciduous membrane presented itself within the tube connected with the chorion. On a deeper incision through the amnion, about a drachm of liquor amnii escaped, and an ovum of about six or seven weeks was plainly visible. The uterus was not at all enlarged or congested, and on laying it open no deciduous membrane was seen. It presented the usual appearance of the unimpregnated state. Dropsy had commenced in the right ovary, which contained about three ounces of fluid. A corpus luteum was present in the left ovary."

The seventh paper is on "*concealed accidental hemorrhage* at the latter end of pregnancy and during labour," by J. Braxton Hicks, M.D. This formidable complication of labour is very rarely met with. Though it is referred to by nearly all the leading writers on midwifery, none appear to have seen themselves cases in which it had occurred; the cases instanced by them being all of them recorded by others. After a "lengthened" research, Dr. Hicks states that he had succeeded in finding memoranda of only fifteen cases. He presents a synopsis of these, and adds the history of eight others, one from his own practice and seven from the practice of his contemporaries.

Of these cases 15 terminated fatally, one in which the hemorrhage occurred at the 7th month of pregnancy; one at 7½ months; one at 8; one at 8½; eight at full term, while in three the period is not recorded.

In the 8 cases which recovered, in one the hemorrhage occurred at the 6½ month of pregnancy; two at 7½; three at 8; one at 8½; and *one* at full term.

The state of the membranes where mentioned, was as follows: in those who died, they were ruptured naturally or artificially in 9; unbroken in 2. In those who recovered they were ruptured artificially in 7; no case is given in which they were not broken.

The age of the patients is recorded in only 13 cases; where the age is not mentioned, some appear to have had many children. Of those who died, 1 was aged 36 years; 2 were aged 38; 4, 40; 1 was aged 42; 1, 48. Of those who recovered, 1 was aged 40 years; 1, 42 years; 1, 32 years; and 1, 23 years.

Referring to the diagnostic symptoms alone; the most prominent and constant is faintness or collapse, which very frequently takes place suddenly. Almost as constantly is observed a great distension of the fundus uteri, causing it to protrude to an unusual extent into the epigastric region, attended with an indistinct, doughy feel, the outline of the fœtus being lost at this part. In about half the cases, a very distressing sensation, "as if about to burst," was experienced by the patient, with or without pain or tenderness on pressure. There is almost always a nearly entire absence of true labour pains on the first occurrence of faintness, and for some time after. In nineteen of the cases recorded, there were labour pains only in three, and in one of these they were very slight.

From rupture of the uterus concealed hemorrhage is very readily distinguished in cases of the former where the fœtus has escaped into the cavity of the abdomen. But when the rent is small and the fœtus has not escaped, the diagnosis will be more difficult. Here the pain attendant upon the rupture of the uterus, and the change in the form and size of the latter contrasted with the increasing enlargement, and indistinct feel of the fundus, which are present in cases of concealed hemorrhage, will indicate that the uterus is becoming filled rather than emptied. The location of the pain, the presence of pregnancy, the absence of any violence or previous disease, the increasing bulk of the uterus, are sufficient to distinguish concealed hemorrhage from rupture of some other internal organ.

Ordinary fainting, a common occurrence during pregnancy and labour, may, when severe and protracted, be mistaken for concealed hemorrhage. The hand placed upon the uterus will detect the peculiar state of that organ, and lead to a suspicion of the cause of the fainting; and the pain at the fundus, when any is present, will increase the certainty of the diagnosis, especially in cases where the patient has not been exerting herself, nor has been exposed to violence, and if the liquor amnii has already escaped.

From an analysis of all the cases referred to by Dr. Hicks, it appears that the more advanced the pregnancy the more fatal does concealed hemorrhage become; and this because of the more easy separation then of the placenta; of the capacity of the placenta, from its increased diameter, to receive and retain beneath it a greater amount of blood without destruction of its marginal attachment, and also from the large capacity of the vascular system, permitting more blood to be effused in a given time than at an earlier period; and, finally, from the uterus being already distended to its normal extent, it becomes, from the increase of its contents, less capable of expulsive efforts, and the uterine sinuses are thereby more patent, in consequence of the arrangement of their falciform pseudo-valves.

In regard to the proper treatment of this formidable accident, Dr. H. believes the indications to be clearly to empty the uterus, and to detach the placenta wholly at as early a period as is compatible with the safety of the mother. From the simple rupture of the membranes we are not, it is true, to expect the same prompt relief as in the ordinary cases of accidental hemorrhage. Still, as the evacuation of the waters tends to hasten labour, we should employ it, if for no other purpose, as well as the other methods now pursued in this form of flooding.

"But when the patient is *in extremis*, it seems best," Dr. H. remarks, "rather

than empty the uterus at once, which may be a fatal operation, to support plentifully with stimulants, rupture the membranes, if not already broken naturally, apply very firm external pressure to the uterus, especially at the fundus, with cold, and wait till a little improvement takes place; as the system revives, secale should be given, or galvanism, if at hand, employed to excite the uterus to contract. Should the parts be dilating or dilatable, and the uterus fail to respond, it seems useless to give any more time for a fresh effusion; therefore, if the child's head be low, seize it with the forceps, or if *in utero*, turn as gently as possible, of course, in all cases keeping the uterus in as active a state as possible by the above means, and by frictions and pressure during the expulsion of the infant, blood, and placenta."

The subject of the next paper is "*Special Position and the Obstetric Binder, as Aids in the Treatment of Impeded Parturition.*" By Robert Hardy.

An abstract of this paper was given in the number for July, 1860, pp. 265–6.

Dr. Graily Hewitt details an interesting case of "*hydatidiform degeneration of the ovum,*" after expulsion of which the patient did well.

The tenth paper gives the history of a number of "cases of *menorrhagia treated by injection*, or by the removal of the uterine mucous membrane by the gouge, or by both means combined." It is from Dr. C. H. F. Routh.

In obstinate cases of menorrhagia, Dr. R. refers to two conditions of the uterine mucous membrane which require to be corrected: one is the presence of polypi of various sizes and number, growing from its surface; the other is a softened condition of it, probably inflammatory in character, which bleeds upon the slightest touch, in many cases upon the slightest movement of the patient.

The injections that have been employed are tr. iodine, diluted or undiluted, a strong solution of tannin, or the tr. ferri muriatis.

Previously to injecting the uterus, it is essential, in order to prevent injury from the retention of the fluid thrown into the uterus, to dilate the latter by the use of the sponge tent for three or four days, until the cavity is large enough readily to admit the index finger. Dr. R. now draws down the uterus by means of a peculiarly constructed, corkscrew-shaped hook, which neither compresses the cervix, nor endangers the tearing of the os. When the entire uterine mucous membrane has been removed by a proper instrument, several of which are described in the paper before us, about half an ounce tr. iodid. co. or tr. ferri sesquichlor. is conveyed by means of an elastic gum catheter to the superior portion of the uterine cavity, and allowed to flow down through a speculum into the vagina. If pain sets in, an opium suppository is applied within the rectum.

"One application of the injection," Dr. R. remarks, "often suffices, but more lately I have repeated, in very bad cases, the injection from three to five or even six days, so long as the os continues patent enough to admit a goose quill high up. After the second or third day, a reddish, mucous discharge begins to flow, where tr. ferri sesquichlor. has been used, accompanied with large, thin, black flakes, which look very like scales of charcoal; this is, however, soon replaced by an ordinary leucorrhœa, and the cure is generally complete. The operation may sometimes fail in those cases where the hemorrhage is due to a softened, probably inflamed, villous membrane, if the whole diseased part of the mucous membrane, or perchance some polypus growing upon it may not have been scraped away. The disease may, therefore, at no distant period, reappear. Now, I believe that in many of these cases the injection, if applied at once, corrects the error of the operator, by constringing, and then setting up a healthy and absorptive action in the lining membrane of the uterus, and particularly in the diseased parts accidentally left behind. Indeed, this absorptive action, it might be said, in some measure, was set up by the very sponge tent itself."

The scraping of the uterus, it is remarked, is not required in every case. Injections, preceded by a sponge tent, and repeated frequently, may obviate the necessity of the gouge.

We have next the history of an interesting case of "*complete obliteration of the canal of the small intestine* by fœtal peritonitis," in which Amusat's operation was successfully performed by R. Druitt.

The twelfth paper is on certain phenomena, facts, and calculations, incidental to, or connected with "*the power and act of propagation in females of the indus-*

trial classes" in London, derived from the experience of two lying-in institutions. Embracing a period of eleven years, during which upwards of 12,000 cases were carefully observed. The paper is drawn up by Dr. A. B. GRANVILLE.

Basing his observations upon 16,258 cases in which the age of the women was ascertained, Dr. G. sets down the average date of pregnancy as $29\frac{3}{4}$ years; adding the multiparous women it becomes 30. The earliest age at which pregnancy occurred was 13 years, of which 5 cases occur. Under *fifteen* years, pregnancy occurred in 15 cases; under 18 years, in 102 cases. The latest period of pregnancy was in the 55th year of age, of which 3 cases are recorded. The entire number of cases in females over 45 years of age, was 59; in females over 46 years, 38; in those over 47, 29; in those over 48, 23; in those over 49, 14; and in those over 50, 11.

Of the single births, out of 16,258 cases of parturition, 6,615 were boys, and 5,863 were girls: total, 12,478.

In respect to the condition of the children born alive, one had one arm shorter than the other; one had a cleft palate—spina-bifida—no testicles externally or internally; impervious urethra; urine in bladder; one had ascites; one had deformed feet; one was variously deformed;[1] one had spina-bifida simply; one had cleft palate, hare-lip, and absence of one half upper jaw; one had depressed parietal bone; another, the same, with the longitudinal edge of the bone pulling the membranes of brain and the scalp, over the longitudinal suture, on the stretch, protruding upwards—left eye also protruding. One child, born of a mother who died on the seventh day of phthisis, weighed *eleven pounds*, and measured fourteen inches.

Of 12,478 births, 277 were still-born—151 boys, 126 girls. Of these, 20—11 boys, 9 girls—were born before the full period of gestation. Fifteen children—4 boys, 11 girls—were born alive before the full time, by several weeks. Three children—1 male, 2 females—were born at 6 months, alive.

In respect to the mortality among mothers, in 12,423 cases of labour, it occurred *once* in 500—the total number of deaths being 25.

The total cases of twins in 12,618 births was 136—of these, both were boys, in 35 cases; both girls, in 51 cases; and a boy and girl in 50 cases. The proportion of triplets was one in 1,051 labours, or a total of 12 cases in 12,618 labours. The total children in these multiple births was 284—128 boys, 156 girls.

The number of pluriparous women who had twins, 136, or 1 in $91\frac{17}{22}$; who had triplets, 4, or 1 in $3,119\frac{1}{4}$. Children produced, 284. Of whom were alive, and at the full time, 252; alive, and before the time, 7; stillborn, 25, or 1 in 10.216 cases.

Instrumental cases in 12,423 cases of labour, 52; number requiring manual interference, 46; number in which the head was perforated, 9. Number of cases in which morbid accidents intervened, 62, namely, hemorrhage, 23; convulsions, 4; locked jaw, 1; syncope, 2; retained placenta, 24; abnormal pelvis, 6; total suppression of uterine action, 2. Proportion of cases with accident to entire number of births, 1 in 200.37.

In 12,423 labours, the presentation was of the head in its normal position in 12,156 cases; in an abnormal position in 21 cases, or, 1 in $591\frac{6}{10}$, of these latter 1 terminated naturally, 4 required manual interference, and 16, instrumental. The face presented to pubis in 52 cases, or 1 in $238\frac{9}{10}$—42 terminated naturally, and 10 were instrumental. The hand presented, with three complications, in 29 cases, or 1 in $438\frac{3}{10}$—7 terminated naturally, 19 required manual interference, and 3, instrumental; in 91 cases, the breech presented, or 1 in $136\frac{6}{10}$—79 terminated naturally, 6 required manual interference, and 6, instrumental; in 54 cases, the feet, with two complications, presented, or 1 in $230\frac{6}{100}$—48 terminated naturally, manual interference was required in 4, and instrumental in 2; in 11 cases, the funis, with one complication, presented, or 1 in $1129\frac{4}{11}$ cases—10 terminated naturally, and 1 required the use of instruments; there were 9 cases of placenta prævia, or 1 in 1380 cases—3 terminated naturally, 5 required manual interference, and *one*, instrumental.

The next paper is the history of a case of "*ruptured perineum, and birth of*

[1] See London Med. and Physiol. Journ., vol. i. p. 175.

the child between the os vaginæ and anus." It is by J. Baker Brown. This accident appears to have resulted from an unyielding condition of the os vaginæ. The relator suggests the propriety in similar cases, at the time of the severe pressure on the perineum, when the os vaginæ shows no disposition to dilate, to divide the latter freely on each side with a blunt-pointed bistoury, so as to prevent the possibility of this peculiar lesion.

We have next the history of a case of "*congenital hernia* (?) *of the liver.*" By Dr. A. Meadows. It is a curious case, but presents no point of especial interest.

The 15th paper is on the etiology of "*phlegmasia dolens,*" by Dr. Wm. Tilbury Fox.

Upon a careful analysis of the causes to which phlegmasia dolens has been ascribed by the different writers who have treated of it, and the various circumstances under which it has been observed to make its appearance, Dr. Fox believes that these lead to the deduction that the disease is occasioned by an impediment to the free return of blood and lymph from the affected part, and that the causes of such impediment may be, so far as regards the vessels, extrinsic or intrinsic. Of this somewhat elaborate and highly instructive paper, we regret that, without exceeding the proper limits of a biographical notice of the volume before us, we can present merely the general propositions assumed by the author, referring our readers to the paper of Dr. Fox, for an exposition of the facts and arguments upon which these are based. The propositions in question are as follows:—

"1. In phlegmasia dolens both veins and lymphatics are obstructed.

"2. The obstruction may be due simply to extrinsic pressure.

"3. Or, to inflammatory changes in the coats of the vessels leading to coagulation (this depends upon virus action). Except during epidemics of puerperal fever, this is not so common as supposed.

"4. It is pretty well admitted that rapid ingress of abnormal fluid suddenly, and in large amount, will cause instantaneous coagulation of the blood; and it is also admitted that large drains upon the system are followed by rapid and compensatory absorption. There is good reason for believing that these conditions are fulfilled, in a perfect and ample degree, in conjunction with the presence of wound—facilitating absorption—in a great many cases, prior to the occurrence of phlegmasia dolens, and that the latter is frequently thus evolved.

"5. These different modes of evolution may be more or less conjoined."

The subject is continued in the next paper, by the same author, on "*the pathological lesion of phlegmasia dolens.*" In this are examined "the essential condition of the limb" in phlegmasia dolens, and the manner in which this condition is produced.

We are unable to follow the writer in his investigation of these two questions; as we have done in respect to the preceding paper, we give merely his general conclusions, viz:—

"1. That obstruction to the lymphatics obliterates one of their functions (complementary in relation to the nutrition of the cellular tissue); the excess of pabulum not being removed, hypertrophy ensues, with retention in the interstices of fibrinous material, all of which are favoured by hyperinosis, hyperæmia, &c. 2. That this is the ample disease—phlegmasia dolens. 3. That a virus may produce obstruction to the venous and lymphatic currents, and thus phlegmasia dolens; but, in this case, eliminative and special reparative actions ensue, which are superadded conditions, not peculiar to, not essentially a part of, phlegmasia dolens, but especially related to the blood-disease, in which phlegmasia dolens occurs."

The 17th paper is the history of an enormous "*fibrous tumour connected with the uterus,*" in the practice of Dr. R. U. West, reported by Dr. Graily Hewitt. The patient was 53 years of age, and died from an attack of uterine hemorrhage. The existence of the tumour had been repeatedly diagnosed for the space of ten years. It was firmly adherent to the abdominal parietes anteriorly, it weighed 42 pounds; its diameter was about 16 inches, and its circumference 44 inches. Bands crossing and constricting the vagina half an inch below the os uteri, prevented the latter from being reached by the finger.

The substance of the tumour was hard and resistant; it possessed a fibrous texture, septa running in various directions. It had been apparently moulded to the shape of the abdominal walls. The section was white, firm, and showed large vessels. No evidence existed of softening at any part—the structure was everywhere uniform in composition. The connection of the tumour with the uterus was very slight; it apparently proceeded from the anterior or the posterior (which, it is difficult to say) portion of the cervical part of the uterus. Firm, fibrous bands passed off from this part of the uterus to the tumour, but the main part of the connecting substance was composed of cellular tissue and large vessels. The connection between the uterus and tumour was not more than three-quarters of an inch in thickness.

The next paper is the history of a case of "*hydatidiform degeneration of the ovum*," by Dr. C. H. F. ROUTH. After suffering from various obscure uterine symptoms, with menorrhagia, for some two years, the patient was finally attacked with severe hemorrhage and regular uterine pains—finally, a large mass of hydatidiform bodies was discharged, together with the uterine membranes. The treatment adopted was the application of cold cloths, and scruple doses of secale cornutum every half hour, with a little cold brandy and water to relieve a feeling of weariness. The recovery was good, all hemorrhage ceasing after the expulsion of the membranes.

The treatment in cases like that here described is evidently to cause the expulsion of the dead ovum, and thus stop the constant drain on the patient's strength. Dr. R. suggests the dilatation of the os uteri by sponge-tents, so as to make out certainly the nature of the body present and to favour its removal or expulsion.

Following Dr. Routh's paper is the description of "*two fœtal monsters*," by Dr. T. H. TANNER.

One of these monsters presented two well developed heads, four upper and four lower extremities; a single large thorax; a single abdomen. The parts of generation, those of a female, were complete in each infant. Dissection had to be delayed until decomposition occurred, but it was easily made out that there was only one thoracic cavity, with two lungs, one heart, and one sternum. The abdominal cavity was likewise single, having but one liver and one spleen, two kidneys, and one set of intestines. There was only one umbilical cord and one placenta.

The second monster was affected with congenital umbilical hernia. There was a large fissure at the epigastric region, above the umbilical cord, the part of the abdomen which naturally remains the longest open, and which, in this case, an arrest of development had prevented from closing at that period when a portion of the abdominal viscera is contained in the sheath of the umbilical cord. Below the umbilical region the integuments were in a healthy condition. The hernial sac was formed of two coats, externally by the amnion and internally by the peritoneum. These had been ruptured, showing the contents of the hernia to consist of the intestines and liver, and at the upper part the heart. The fœtus was dead when born.

We have next the account of a case in which a "*fœtus* was *retained in utero for some weeks after its death and decomposition*," reported by Drs. NEWMAN and HARLEY.

From the history of this case we learn that a fœtus, conceived in November, after passing through four months of uterine life without any unusual occurrence, died in March from some non-evident cause, and, yet dead and decomposed, was retained in utero for three months more; its ultimate expulsion being determined, not by the uterus resenting the presence of a lifeless mass, but by the accidental separation of a portion of the placenta from the uterine wall, with consequent hemorrhage into the substance of the placenta, appearing also externally; by this, uterine action was induced, and the subsequent extrusion of the contents of that viscus.

A case of "*extra-uterine pregnancy*" is narrated by Dr. CHARLES DRAGE. The case is an interesting one. The patient considered herself pregnant, and expected her confinement to take place in June or July, 1852. In January, 1852, suffered from violent attacks of pain principally in the region of the transverse colon,

with obstinate constipation and constant vomiting. Catamenia occurred three times regularly before the end of March, when there was another attack of pain. Between the end of June and beginning of August a good deal of hemorrhage occurred at intervals. From this time onwards the catamenial periods were quite regular. Intumescence of abdomen as in pregnancy. Uterus felt larger than in the unimpregnated state. Placental bruit heard distinctly, but no sound of fœtal heart. Cervix uteri shortened, as at the eighth month.

From the month of June the abdomen gradually diminished in size, and by June, 1853, was nearly in its natural condition. There was always, however, a distinct tumour to be felt deep down in the pelvis, but which caused no pain or uneasiness. The patient continued very well all the autumn and winter; in January, 1854, she went on one or two occasions to a dance. From February, 1854, she suffered from morning sickness, and had had an offensive discharge from the vagina for several weeks previous to the middle of March, when, upon examination, an oval opening with rounded edges, and of a size to just admit the point of the finger, was detected in the posterior wall of the vagina: an examination per rectum disclosed an opening in its anterior wall corresponding with that in the vagina, and similar in character. Through both openings a mass lying between them and feeling like bone, was made out. There was entire loss of appetite, constant vomiting, and great emaciation. She was sent into the country and put on sherry wine and pale ale. By the 27th of April, 1854, the patient having somewhat improved in health, Dr. Drage, after dilating the vaginal opening with his finger, extracted, by means of a pair of lithotomy forceps, after a good deal of trouble, a quantity of fœtal bones—phalanges of fingers and toes, and the left temporal bone, entirely free from muscle. On various occasions, up to May 20th, he removed others, and on that day a large putrid mass, being apparently the spine and ribs, with the dorsal muscles still attached. He had now obtained an almost complete fœtal skeleton. By June 8th, nothing more was to be felt. The finger passed freely into the rectum, and upwards into the sac. The openings had considerably contracted. It was advised that a small piece of sponge should be worn in the vagina, to prevent the fecal matter entering from the rectum. By June, 1860, the patient was perfectly well, and much stouter than at any time previously. Experiences no inconvenience from the openings, save when the bowels are much relaxed, and then only a very small quantity passes per vaginam.

The twenty-second paper is on "*some of the exigencies connected with preternatural labour.*" By Jos. T. Mitchell. The object of this highly instructive paper is to point out some of the quicksands into which the obstetrician is liable daily to fall in the course of his ordinary duties, and the common rules to guide him in their avoidance. These rules we shall give entire, remarking, that the whole of Mr. Mitchell's remarks are worthy of a careful perusal on the part of every obstetrician, especially of those who have recently entered the field of obstetrics. His rules for avoiding the quicksands referred to, are:—

"1st. He should never omit to inform himself accurately as to every particular connected with every former delivery, when his services are engaged in any case in anticipation of an approaching labour, so that he may be quite prepared to act judiciously, as circumstances may dictate, either before the event, or at the time thereof.

"2dly. He should carefully educate himself, so that he may acquire a good and accurate diagnostic touch—absolutely necessary to guide him in ascertaining accurately, early in labour, the true nature of the presentation, as well as the capacity, normal or otherwise, of the bony passages—a desideratum essential in every good accoucheur.

"3dly. He should take care never to allow of the possibility of any evil consequence following an unnecessarily protracted labour, which he had the means of preventing by adopting early and judiciously applied manual or instrumental aid.

"4thly. He should never omit to induce premature labour in cases where permanent mechanical interruptions in labour at full time were previously known to exist, by puncturing the membranes at the legal period of seven months and

a half, or at eight months' gestation, which can so easily and safely be done by using a proper instrument for that purpose.

"And, 5thly. In the present improved condition of instrumental appliances for delivering in craniotomy, &c., he should never rely on the use of such an instrument as the crotchet, which, in the hands of even reputedly expert operators, has been known often to produce great injury, sometimes to the operator, but more frequently to the patient."

In the next paper we are given by Dr. JOHN H. DAVIS, a description of a "*double battledore placenta, with a single umbilical cord, connected with one child.*" The case is curious, it is true, but of little practical importance.

Then follows the history of a case of "*sudden apoplectiform seizure, terminating fatally in thirty-five hours, on the sixth day of lying-in.*" By Dr. R. UVEDALE WEST. The seizure occurred unexpectedly, after a very easy, quick labour, without any appreciable exciting cause. The symptoms were not certainly those of apoplexy, properly speaking. The patient was semi-comatose, and affected with only apparent loss of voluntary power in the limbs of the right side. The head only was examined after death. The appearances detected are thus described:—

"The dingy, dirty, freckled complexion of face was now more striking than during life. On incising the scalp down to the pericranium, there was a considerable escape of gas. There was a patch of extravasation as large as a crown piece inside the scalp, covering the left side of the occiput, and a similar smaller patch in the same situation on the right side." These were possibly bruises the patient had inflicted on herself in her turbulent movements. "The skull was unusually thin. The brain was much congested on its surface, but its tissue was healthy, with the exception of some small patches of softening outside of the left choroid plexus. The lateral ventricle on that side was empty, but the right lateral ventricle contained a considerable quantity of serum of a slightly bloody tinge. There was no extravasated blood anywhere in the brain. The cerebellum was healthy throughout."

Paper twenty-five is the account of a case of "*ovarian tumour weighing seventy-six and a half ounces, in a girl of twelve years and a half old,*" terminating life suddenly by asphyxia. It is related by R. FAWCETT BATTYE.

The cause of death in this case is set down as evidently asphyxia, arising from the unyielding nature of the abdominal walls, not allowing the tumour to become more prominent anteriorly, and consequently the pressure, taking the line of least resistance, was directed upwards towards a very enlarged liver through the medium of impacted intestines.

The twenty-sixth paper is "*An Inquiry into the Correctness of the Doctrine of William Hunter in regard to Retroversion or Retroflexion of the Gravid Uterus.*" By Dr. W. TYLER SMITH. A full abstract of this paper was given in our number for January of the present year, pp. 279–80.

A curious case of "*spontaneous evolution of the fœtus in utero,*" in which the head changed for the feet, is related by Dr. RICHARD HODGES. It was a first labour. Upon the first examination, the head was clearly and distinctly made out as the presenting part. After an interval, a second examination was made, when both feet were found in the upper part of the vagina, just through the os uteri, the child having actually revolved within the uterus.

The next case is an instance of the serious effects of "*reckless vaccination,*" by ROBERT DRUITT. A healthy infant was vaccinated with lymph brought to the house on points. The operation was performed by making a series of parallel scratches close together, and covering a space of one inch by three-quarters. The arm inflamed severely, swelled from shoulder to elbow, and numerous small vesications appeared on the inflamed skin. The part operated on finally sloughed, leaving a very deep and ugly cicatrix.

The twenty-ninth paper is the history of a case of "*abnormal gestation,*" by ROBERT HARDY. A lady twenty-two years of age, in the seventh month of pregnancy, was attacked with pain in the left side of the abdomen, shooting down toward the pubis, which was relieved by an opiate and laxative, with perfect quiescence. Some days later the pains returned with increased violence, with frequent fainting fits. Upon a careful examination of the patient, the uterus

was found high up, its cervix an inch or more in length, conical, tapering off to the os, which was unusually pointed, and in contact with the sacrum. The inferior segment of the uterus was quite undeveloped, the organ to all appearance empty. External careful manipulation discovered a large oblong tumour in and extending obliquely upwards from the left hypogastrium to the crista ilii of the same side. All the pain was experienced in this situation, and did not extend across the pubis. Anodynes, camphor mixture, hot dry local applications, and aperients, produced little relief; pain and frequent syncope still continued.

Some ten days later the faintings had become less frequent. The pain of the left side constant, with now occasional exacerbations. The only alteration detected by examination per vaginam was increased moisture of a thin colourless character. (Qu. *liquor amnii?*)

Three days later abdominal pains rather increased, with more severe exacerbations of a somewhat expulsive character. During a pain the abdominal tumour became defined and hard, extending from the pubis obliquely upwards and outwards, and ending by a rounded margin within the left hypochondrium. Two days subsequently the pains were of a more aggravated character, assuming a decidedly parturient aspect. Examination per vaginam showed no change in the condition of the os, cervix, and lower segment of the uterus. The pains recurred every eight minutes; the tumour, without altering its shape and direction, became very hard. There was now detected a narrowed part crossing the tumour, about an inch and a half in breadth, and two-thirds below its summit. Towards midnight the pains, which had abated towards evening, became greatly aggravated. Next morning, being summoned in haste, Mr. Hardey found that a neighbour had drawn away a fœtus, which she said had come double, and was nearly born when she arrived; the placenta had been expelled naturally, it was said, just before Mr. H. entered. The fœtus was of seven months' development, and had been dead many days. There was no hemorrhage, and the cervix uteri had the same long conical shape, with the pointed os, as before. A lateral slit was detected extending through the cervix, quite up to the body of the uterus, as if it had been cut clean across with a knife. After some gentle kneading, the uterus was roused into action, when it was felt occupying the left iliac region, of the size of a small cocoa-nut. The mother speedily recovered, without the occurrence of any untoward symptom.

Mr. H. now learned, for the first time, that the late gestation was not, as he had at first been led to believe, her first. Early after marriage she had become *enceinte*. Towards the end of the third month of gestation her chair was removed as she was in the act of sitting down, causing her to fall suddenly to the floor; she was attacked with severe pain in the left groin, and aborted the next morning. Her health continued bad for three months, with more or less pain for several weeks in the groin. Soon after her recovery she became again pregnant, suffered at intervals considerable pain in the left side, and aborted a second time at the end of the third month. Her *third* gestation, with its results, forms the subject of the present case.

Mr. H. presumes that when the sudden fall occurred to the patient during her first pregnancy some lesion occurred in the peritoneal covering forming the left broad ligament, and extending to the uterus, the result being plastic effusion, forming in time an organized band of a comparatively inelastic nature, by which the uterus was prevented from rising upwards as gestation advanced, and giving it a direction to the left, forming ultimately the stricture by which the uterus became divided into two chambers, and imprisoning the ovum in the upper one till within four hours of delivery. This firm band across the lower third of the uterus would seem to account, also, satisfactorily for the undeveloped condition of the lower third of the organ, with its cervix and os. The advent of a labour at the close of the seventh month was evidently caused by the impossibility of any further expansion of the uterus, while its great severity and protraction were also due to the same cause.

The next paper is the history of a case, by Dr. R. Barnes, showing the influence exerted by a shortening of one leg from early age in inducing a "*symmetrical distortion of the pelvis.*"

In the case described, it is clear, that the main support of the body in the erect position, and in locomotion, was by the sound right leg. The muscles of the right side of the pelvis assumed, in consequence, a more active development, and caused a corresponding development of the bones to which they are attached. At the same time, the left side of the pelvis, without being actually atrophied, was very slowly developed. The result was a fully, perhaps an excessively, developed right half of the pelvis, a contracted left half, a narrowing of the conjugate diameter, and oblique distortion of the brim, and a pelvic cavity shallower on the left than on the right side. Dr. B. is inclined to believe that deformities similar in kind, but less in degree, the result of lesser degrees of "clandication," or of unequal muscular power of the legs, may not be uncommon.

Dr. J. G. SWAYNE describes a case of "*double monstrosity.*" The junction of the two fœtuses appeared to extend from the umbilicus to the top of the thorax. The left fœtus was of full size, the right much smaller. The right arm of right fœtus, and the left arm of the other, were well formed, and widely separated. The two other arms were united together as far as the wrists, and above the elbows they were inclosed within the integuments of the thorax; the hands were separate. Both children were males. On dissection it was found that the ribs of each fœtus were united to a common sternum, to which were articulated, also, the four clavicles, and attached the four sterno-cleido muscles. There was but a single thorax, containing a single pericardium inclosing two perfectly distinct hearts. The only direct venous communication detected was a large branch which passed from the left innominata of one child to the right vena innominata of the other. The lungs and other thoracic viscera were those of two children. The ribs on the posterior side of the thorax were not fully developed; the free extremities in one fœtus lay close to, but not connected with, those of the other. There was a single diaphragm, and a large liver common to the two fœtuses. The single umbilical vein proceeded from the umbilicus to the anterior margin of the liver, and subdivided posteriorly to join the two inferior cavæ. The umbilical cord was composed of a single vein and four arteries. All the abdominal viscera, with the exception of the liver, were double. The two upper extremities that were united together, consisted of two clavicles lying side by side, and quite close together, and articulated to the common sternum. There were two scapulæ; and two humeri. These latter, however, were united inferiorly, forming "a single articular surface for the two ulnæ, which were united above, but distinct at their carpal ends." Between the two olecranon processes there was a deep groove for the ulnar nerves. Besides the above, there were no other anomalies in the condition of these extremities, excepting that they were inclosed in one common integument as far as the wrist.

A very interesting case of "*pelvic cellulitis* after a first pregnancy, followed by suppuration at the back and front parts of the vagina," is related by Dr. GEORGE D. GIBB. Nearly eleven weeks after the first symptoms declared themselves, an abscess pointed at the lower and back part of the vagina, and spontaneously evacuated itself, with complete contraction of its walls. Some matter also found its way to the anterior wall around the urethra, causing, no doubt, much of the difficulty of micturition complained of by the patient. The quantity of matter discharged did not exceed several ounces, to which fact Dr. G. attributes the successful result of the case. The strength of the patient was supported throughout by a liberal diet, and wine, with tonic and anodyne remedies. The patient, during the attack, could take nothing warm—all her food had to be swallowed cold. The catamenia appeared seventeen weeks after confinement, and continued regularly. Pregnancy subsequently took place, and a second child was born nearly seventeen months after the birth of the first, without any inconvenience or bad symptom whatsoever.

A case of "*annular laceration of the cervix uteri*," is related by Dr. T. HERBERT BARKER. It occurred in a patient forty-two years old, during her first labour at full term. The term annular is employed because a complete ring-like portion, consisting of the os and cervix, was torn off from the body of the uterus; the child having passed through the laceration and not through the os uteri.

"The cause of this kind of laceration is," Dr. B. remarks, "the protracted pressure of the head against a ring of the uterine parietes in a contracted pelvis;

perhaps also conjointly with a sharp linea-ileo-pectinea. An early discharge of the liquor amnii will, of course, tend to aggravate the mischief. The particular feature in the case related was the undilated os uteri—and the loose condition of the os and cervix uteri in front of the head—even to an advanced period of the labour. * * * It is probable that this will be found to be characteristic of cases of annular laceration, for it is in this way likely that the pressure of the head is concentrated upon a circle of the uterine walls, corresponding with the margin of the pelvis, instead of being partially expended upon the surface of the uterus below this point. The head is, in fact, impacted in the pelvis, pinching the walls of the uterus between it and the pelvic bones, but all parts of the uterus below this pressed ring and the head, are lax, and unaffected by the uterine pains. The head is continuously, and with immense force, pressed against the girdle of uterine wall, so that the parts are either cut through or thinned, softened, and sphacelated. The separated portion of uterus in this case had the appearance of having been thus pinched and thinned. The substance of the jagged edge was thinner than the structure of the uterus intervening between it and the margin of the os uteri."

Dr. B. thinks that the period at which the laceration occurred was that when, on the third evening of labour, the patient was seized by severe, continuous, and peculiar pain in the lower part of the abdomen, followed by the flagging of the true uterine pains. There was not the sudden cessation of pain as in rupture of the body of the uterus, nor was there prostration of strength, nor vomiting, but the pulse continued strong, undisturbed, and the patient in good spirits.

The 34th, and last paper, is on the "*value of anæsthetic aid in midwifery*," by Dr. Chas. KIDD. The paper is, upon the whole, a judicious one; pointedly in favour of the employment of chloroform in all prolonged and very painful labours; in cases where version or the use of instruments becomes necessary; in puerperal convulsions, etc. Dr. K. is not in favour of the indiscriminate administration of chloroform merely to allay pain in second, third, or fourth cases of labour where, from our knowledge of what happened in the preceding labour or labours, there is every reason to believe that the case will terminate shortly and favourably; he would restrict its use to first labours, or to such as are tedious and painful, or where manual or instrumental interference is required to conduct them to a favourable close. Dr. K. prefers chloroform to all other anæsthetics, and makes the startling assertion that "death occurs as often from ether as from chloroform." That very few accidents had happened from the use of ether, especially few indeed when compared with those which had resulted from the administration of chloroform, was a fact we had believed very generally acknowledged. We know that such is the impression of the medical men of this country. We regret that Dr. K. has not indicated the data upon which his declaration is based.

D. F. C.

ART. XIX.—*On Scrofulous Diseases of the External Lymphatic Glands; their Nature, Variety, and Treatment; with Remarks on the Management of Scrofulous Ulcerations, Scars, and Cicatrices.* By P. C. PRICE, F. R. C. S. E., Surgeon to the Great Northern Hospital, the Metropolitan Infirmary for Scrofulous Children, at Margate, &c. Illustrated with engravings on wood. London: 1861. 12mo. pp. 159.

A MONOGRAPH is here presented, based chiefly on the practical experience of its author, upon a subject whose importance is not lessened by its every-day familiarity to all practitioners. Although drawing chiefly upon his own observations at the hospital at Margate, Mr. Price has not neglected those of others, as recorded especially in the works of Wiseman, Lugol, Baudelocque, Guersent, Hufeland, Philips, Lebert, Tyler Smith, Ranking, Lloyd, Henning, and Goodlad.

Few terms in medical language are used more vaguely than *struma* and *scrofula*. Nor does the indefiniteness of these words, and of their corresponding adjectives, seem easy of removal. Mr. Price objects to the use of the terms *scrofulous* and *tuberculous* as convertible or synonymous.

"The term scrofulous may be applied to disease of a gland in which tubercle plays the chief part; but the word tuberculous cannot with the same propriety be used to denote the nature and characters of every so-called scrofulous gland which, although appearing in what is termed a tuberculous subject, is in no way dependent for its morbid alteration on true tuberculous deposition."

"The term scrofulous must therefore, sometimes, if not always, be employed to include both tuberculous and non-tuberculous diseases."

Affections of the lymphatic glands, skin, mucous, cellular, and other tissues, bones, joints, and various internal organs, characterized by a tendency to inflammation, ulceration, and suppuration, modified by inherited or acquired diathesis, are those to which, in compliance with usage, the name of strumous or scrofulous is applied in this work. The author's attention is confined, however, almost exclusively to disorders of the lymphatic glands.

The causes of the scrofulous diathesis are recognized as being all influences which depress vitality by withholding its necessary conditions of pure air, sunlight, warmth, and wholesome food. Thus formed, the diathesis is capable of transmission by parentage. The opinion of many of the older writers, as very strongly stated by Hufeland, that *syphilis* is the direct source of the scrofulous habit of constitution in many instances, is considered by our author to have been exploded.

Glandular affections are treated of very fully in this work, under the heads of—

I. Acute, Subacute, and Chronic Inflammation, or Lymphadenitis.

II. Chronic Enlargement, or Simple Hypertrophy of Structure, independently of Vascular Derangement.

III. True Tuberculous Alterations.

The distinction between these forms of glandular disease is insisted upon as being very important, not only for prognosis, but also with a view to treatment. It is also of consequence to distinguish between scrofulous disease of the glands and that which accompanies leucocythæmia. The greater proportion of so-called transitory scrofulous swellings of the neck, abdomen, and other parts, is believed to be caused by subacute or chronic inflammation of the glands. More than two-thirds of the cases of idiopathic glandular disease observed by Mr. Price were confined to the glands above the shoulders. Next in liability were those of the axilla; while those of the groin were affected in extremely few instances. Neither Louis nor Lebert mentions a single instance of tuberculous inguinal gland.

In Mr. Price's experience, glandular disease is decidedly more frequent, in early life, in males than in females. Of fifty tabulated cases, thirty-three were males, and seventeen females.

Other interesting statistics are given in this volume; as showing, for example, how rare is the supervention of glandular enlargement upon pulmonary phthisis; and that, although decidedly uncommon, the commencement of scrofulous or tuberculous disease of the glands sometimes occurs after fifty years of age. A few fatal cases are mentioned, in which great enlargement of the deep-seated glands of the neck caused death by impeding the functions of respiration, circulation, and deglutition. Of 140 tabulated instances of supposed tuberculous disease of glands, 82 proceeded to suppuration.

The comparative frequency of the occurrence of true tubercle in enlarged or suppurating glands, has been variously estimated by different authors. At the Hôpital des Enfans Malades at Paris, M. Guersent states that tuberculous disease has been found in one or more parts of the bodies of two-thirds of the scrofulous children dying between the ages of three and sixteen. Papavoine asserts that, of fifty scrofulous children examined, tubercle was discovered in the cervical glands of twenty-six. The proportion in the autopsies of Louis was one-tenth, in the bodies of those dying over the age of fifteen years.

The form of tubercle which occurs in the glands is almost invariably the yellow and "lardaceo-caseous," which seldom undergoes any metamorphosis except that of liquefactive degeneration. *Withering* occasionally takes place; cretifaction still more rarely.

Minute description is given by Mr. Price of the peculiar *cicatrices* of suppurating or ulcerating scrofulous glands. Often purple in colour, they are

generally furrowed, wrinkled, bridled or puckered, and covered by a thin and imperfectly developed cuticle. Sometimes, on or between small arches or bridles of skin, they display buttons or threads, each surmounted by a black spot, apparently an abnormal product of follicular secretion. They are closely imitated by some slight cicatrices following burns of the face or neck.

We have, of course, the best reason to hope for useful information from the records of a special hospital, in regard to the *treatment* of scrofulous affections of the glands. Mr. Price has been, apparently, a careful observer, and has formed conclusions the study of which, with the practical reasons therefor, might be of decided advantage to many physicians.

Referring the reader to the book itself for their detail, we may state, that he regards hygienic management as of the greatest consequence; pure, and especially sea air, and bathing, with good diet, warm clothing, and sunlight, doing, in the Children's Hospital at Margate, very much towards the cure of all forms of scrofula.

In the therapeutics of glandular disease, the internal use of appropriate medicines appears to him to deserve the greatest confidence. Iodine, in his experience, is, *most generally*, of real service, especially in incipient or non-tuberculous struma of the glands, or, when tubercle exists, before it has undergone softening. So considerable, however, is the number of cases in which it will altogether disappoint, that it is in vain to place in it blind confidence as a specific. The usual combination of iodine with an alkaline metal, is said to be justified by comparative practical results. Mr. Price prefers the iodide of ammonium, recommended previously by Dr. B. W. Richardson, even to the iodide of potassium; the dose of the former being rather smaller, and its action more rapid. Intolerance seldom follows its administration. The symptoms, when it occurs, are violent pains in the throat, and heat in the stomach. With either of these salts, however, our author's experience, like that of Lugol, Baudelocque, and Ranking, is in favour of *small doses* long continued. Of iodide of potassium, for example, he gives, to a child, a grain, or half a grain, twice or thrice in the day, on an average; combining it, however, generally in use, with half the quantity of iodine, as in the formulæ of Lugol.

In any mode of employment, Mr. Price has met with no confirmation of the statements made by M. Rilliet, of Geneva, of the dangers of iodism. He has never known an instance of the drug exerting any destructive influence over the normal tissues of the body; although overdoses often produce nausea, giddiness, browache, lassitude, and mawkishness of the breath.

The commonly accepted opinion, that the constitutional influence of mercury is unfavourable to the cure of scrofulous disease, has been fully sustained in the observation of Mr. Price at Margate. The temporary use, especially of the *hydrargyrum cum cretâ*, is advised by him, at the onset of the treatment of many cases, in which the functions of digestion and secretion are at fault.

The ordinary view, that iron, mineral acids, vegetable bitters, and purgatives, are useful in scrofula only as analeptics and palliatives, without any of that effect which (as in the case of iodine) is called *specific*, is maintained in this work.

Cod-liver oil is believed by the author to be of great value; especially in that stage of tuberculization at which iodine proves the least serviceable; viz., when suppuration and ulceration have impoverished the system. He has administered it often, however, with decided advantage, at a much earlier period of glandular disease.

Lime, even in the simple form *liquor calcis*, he considers to be of use, particularly in the later stages of tuberculous degenerations. Of the hypophosphite of lime, in the treatment both of osseous and glandular scrofula, he speaks in terms of strong commendation. The compound of several phosphates, known under the name of "chemical food," mentioned as being "obtainable only from America," is also alluded to with favour, as having been serviceable in a number of instances.

As to local treatment, Mr. Price makes many useful practical remarks which could not be condensed into a sufficiently brief space for this notice. We will call attention only to his views upon the external application of iodine, and the surgical employment, in diseases of the glands, of caustics, and of the knife.

Iodine he believes to be capable of beneficial effect by its local as well as general influence; but not in the way in which, upon mere *routine*, it is commonly employed. He objects to the *unguentum iodinii compositum*, and still more to the tincture of iodine, as being *too irritating* to the skin. We are well satisfied, from frequent observation, that this objection is well founded; and that the former should be much diluted for use, while the latter might be, for external application, at least to scrofulous glands, dispensed with altogether. Mr. Price recommends an unguent composed of a scruple or a drachm of iodide of ammonium with an ounce of spermaceti cerate.

The employment of caustics before suppurative discharge has been established from the gland or glands affected, is not approved; unless, in tedious cases of enlargement, the mere introduction of small portions of *potassa fusa*, to produce a carefully limited destruction of substance. In obstinate glandular *ulcerations*, however, whether superficial or accompanied by extended sinuses, the application of caustics may be very useful. Either nitrate of silver, potassa fusa, or a strong solution of iodine may be used, according to the nature of the case.

Upon the question as to how soon a suppurating or softened tuberculous gland should be opened by the knife (caustic for this purpose being not approved), Mr. Price expresses himself in favour of waiting until fluctuation distinctly proves the presence of fluid, with some attenuation of the skin; but not longer. He holds it to be important to allow as small a part of the integument and neighbouring cellular tissue to be involved as possible. To this end, he sometimes taps a glandular abscess by the subcutaneous method, from a sound part; and, in opening a softened gland, confines himself to a linear incision of small extent, unless the contents of the swelling are not thus removable.

Disfiguring *scars* or *cicatrices* of scrofulous origin may, with perfect safety, and advantage in many cases, be destroyed or modified, either by *potassa fusa* or the knife. Much enlarged glands, either tuberculous or simply hypertrophic, may also, in well selected instances, be removed by excision; although the disease will sometimes return in the glands that are left. H. H.

ART. XX.—*Medical Jurisprudence.* By ALFRED SWAINE TAYLOR, M.D., F.R.S., Fellow of the Royal College of Physicians; Hon. M. D. Univ. St. Andrews; Member of the Royal College of Surgeons; and Professor of Medical Jurisprudence and Chemistry in Guy's Hospital. *Qui nescit ignorare ignorat scire.* Fifth American, from the seventh and revised London edition. Edited, with additions, by EDWARD HARTSHORNE, M. D., one of the Surgeons to the Pennsylvania Hospital. Philadelphia: Blanchard & Lea, 1861. 8vo. pp. 714.

A NEW edition of this standard text-book will, doubtless, be received with great satisfaction. The progress of experience in medical jurisprudence has been sufficiently rapid in this country to insure a demand for its most popular exponent at least equal to that for the London original. The absence of material competition, however, and the intense interest excited among British general readers and professional men by a succession of leading poison cases, have led to the exhaustion of a whole English edition since the last American reprint was issued here.

Another has been needed in this country for some time past, and has been delayed in its appearance only by the desire to present the forthcoming English edition, which Mr. Taylor had kindly promised to forward in advance to his American publishers. This edition, therefore, has been printed under the supervision of the American editor, directly from the advance sheets of the London publication, and comes out simultaneously with the London volume. The similarity of date, of course, has rendered unnecessary, on the part of the American edition, any references or other matter from European sources, as well as a large number within Mr. Taylor's reach from this side of the ocean. A considerable number of short notes and American cases have been added nevertheless, and the whole work has received at the hand of its experienced editor the usual

close revision so necessary in the conduct of such matter through the press, at such a distance from the watchful care of its accomplished author.

Since its first appearance in 1844, as we learn from the author's preface to the last edition, no less than 15,750 copies have issued from the British press. This great success has encouraged him to persevere in his efforts to maintain the work on a level with the progress of medical and legal knowledge; so that notwithstanding very great changes and improvements required in the fifth edition, published in 1858, by the large accumulation of materials, many new cases and observations have been added, and every chapter has undergone a close revision.

Owing to the intervention of this edition of 1858 between the last American edition of 1855 and the present one, the improvement in the fifth American is very great in many respects, both as to arrangement and selection of cases, and in regard to many questions and precedents, which are much better understood than formerly, both in medicine and law. In fact, as the author truly observes, "when compared with the early editions, this may be regarded as a new work." Not that the principles or facts of the science are materially changed; "but during the last fourteen years, improvements in medicine and jurisprudence have taken place to so great an extent that a practitioner, whether of the medical or legal profession, would in the earlier editions, meet with deficiencies which on the present occasion it has been my special object to supply." We need not say that the same comparative progress, due in part at least to Dr. Taylor's own teaching through his successive publications, has been going on even more extensively, because more needed, in this country; while the American materials in the shape of cases and decisions as well as able works and papers, have been correspondingly increasing in amount and importance.

"In the section on POISONS, a modification of the definition of the term poison, and of the act of poisoning, has been rendered necessary by crimes of recent date. Additions have been made on poisoning by ammonia,—chronic poisoning by arsenic,—the absorption and detection of arsenic in the dead body,—poisoning by arseniuretted hydrogen,—the detection of absorbed mercury,—poisoning by Scheele's green, tartar emetic, locust beans, prussic acid, nux vomica, and strychnia. The chapter on the two last-named poisons has been entirely rewritten." The section on poisons has been still further condensed and shortened, the details being "given throughout in a concise form, as constituting only a part of the general science of Medical Jurisprudence."

For "that special information" on facts, whether of a legal medical or medico-legal kind which belong to the subject of Toxicology," he refers to the late edition of his work on Poisons, since republished by Blanchard & Lea.

"In the section on WOUNDS, the additions include the rules respecting dying declarations made to medical men,—the detection of blood on weapons and clothing,—the medico-legal examination of wounds,—the microscopical and chemical analysis of blood,—cicatrices,—locomotion after severe injuries,—and the effects of concussion of the brain and spinal marrow, illustrated by recent cases.

"Additional facts have been introduced into the sections on PREGNANCY, DELIVERY, and ABORTION; and new cases are appended to the subjects of Tenancy by Courtesy, Protracted Gestation, and LEGITIMACY. In the chapter on RAPE, additions have been made to the medical proofs of rape on infants and children.

"The chapters on DROWNING and other forms of death by Asphyxia will be found to contain new facts and cases; and, lastly, in the chapters on INSANITY, the sections on Homicidal Mania and Dipsomania have been corrected and enlarged."

The volume has been very accurately and handsomely printed; and, although so much improved in the character of its contents, is very little if any more bulky than in previous editions; in this respect, as in others, affording evidence of the judgment and skill of all concerned in its getting up for the use of the American reader. A material improvement on the English edition, for which we are disposed to be thankful, is to be found in a full list of contents and a much better index.

ART. XXI.—1. *A Knowledge of Living Things, with the Laws of their Existence.* By A. N. BELL, A. M., M. D.; late P. A. Surgeon U. S. Navy; Physician to Brooklyn City Hospital. New York: Baillière Brothers, 440 Broadway, 1860.

2. *The Physiology of Common Life.* By GEORGE HENRY LEWES, author of "Sea Side Studies," "Life of Goethe," etc. In two volumes. New York: D. Appleton & Co., 443 & 445 Broadway, 1860.

3. *Studies in Animal Life.* By the same author. New York: Harper & Brothers, 1860.

THOUGH designed chiefly for popular reading and study, the subject-matters of these works are such as to render an examination of them appropriate in a *Medical Journal.* They have one common aim, viz: the instruction of the community in a branch of science which is attracting increasing attention. We have long regarded instruction therein, not only as legitimate in the higher class of seminaries and colleges, but as a subject which should more extensively engage the attention of medical men, for their own benefit, as well as for the general good.

1. The first book on our list both in design and execution, with some exceptions, which we may presently allude to, conveys much valuable and interesting knowledge, and is well adapted to promote a regard for natural science and personal hygiene. It may be said to consist of two parts. In the first, the reader is presented with a succinct view of the progress of animal and vegetable life, from the most simple to the most complex forms of organization.

Starting with a description of the very first rudiment of a living being, *the cell*, a clear and comprehensive account is given of the mode in which that simplest form of life is gradually developed, with successively higher forms of organization, until the highest and most complex of all—the *mammal.* The various stages of increase in organization are happily illustrated, both by references to animal and vegetable objects, and abundant pictorial illustrations, rendering the elemental operations of life familiar to ordinary minds, and, in our judgment, despite the assertion of the author to the contrary, both "entertaining to the general reader," and "comprehensible to the academic student."

The first part of the volume (according to *our* division of it) concludes with a chapter on "The Consummation of Organic Development," whence the step is direct to a consideration of the conditions which sustain "the erect stature and expressive countenance of *man*," and the remaining half of the volume is devoted to a brief synopsis of human anatomy and physiology.

A rapid and "sketchy" account is given of the various tissues, the cellular, the adipose, the muscular, the osseous, the cutaneous, the nervous; and following these, the subjects of aliment, digestion, circulation, and respiration, have each a few pages devoted to them, while a comparatively minute and protracted exposition of the *senses*, claims a much larger share of attention, and the volume concludes with a philosophical disquisition on "our functions and faculties," and what is rather singularly denominated "the sum of life," as it is rather an essay on disease and death than life. Passing what we regard as an error very common in works of this popular character, the undue proportion of space devoted to a comparatively unimportant point of physiology and anatomy—the senses—we have to allude to one or two other matters which deserve a more accurate rendering than is here given them, especially as they pertain to questions which are at the present time pretty freely and frequently discussed in general circles. Thus, speaking of the means usually employed in warming dwellings, we find on page 192 the following:—

"Water is placed near furnaces *ostensibly* to supply moisture, it being assumed that the highly heated air by furnaces is deficient of (in?) natural moisture and too dry for healthy respiration. Such air is, indeed, very injurious and very irritating, but the remedy is worse than the disease."

In this we differ from the author *in toto.* It is impossible to heat the atmo-

sphere of a dwelling, by any artificial means whatever, without a greater or less desiccation of the air, and unless the increased and unnatural dryness is compensated by vapour from some artificial source, the atmospheric demand for moisture will be supplied, to a greater or less degree, from the lungs and skin of those who inhale the over-dried air. It is the rapid evaporation from the cutaneous and respiratory surfaces that principally gives to the ordinary furnace-heated air its harsh and uncomfortable sensation, a result which can only be obviated, either by a rapid ventilation, and the influx of copious quantities of fresh external air, or by the artificial addition of moisture, by the evaporation of water placed in the air-chamber of the furnace, or better still the occupied apartment.

Dr. Bell very properly objects to the practice of too closely shutting the damper of a furnace or stove, but he gives as the reason, that "burning coals, after they have ceased to smoke, always give off carbonic acid in large quantities," which contaminate the air of the apartment. We would suggest that if anthracite coal is here alluded to (and this is almost the only fuel used in furnaces and stoves in this country), it gives off no *smoke* while burning, and the gaseous products of its combustion consist largely of carbonic *oxide*, as well as carbonic acid. The pungent larynx-irritating, cough-exciting ingredient of the air from a leaky furnace is chiefly the former; that which burns with a lambent blue flame, on the surface of an anthracite fire.

In the chapters on "Aliment and Digestion," and on "Respiration," we have a lucid statement of the most important points of these most important processes, well adapted to ordinary comprehension; but there are a few paragraphs which, either from a want of clearness of expression, or some other cause, convey erroneous impressions. Thus, on p. 226, paragraph 541 contains as many inaccuracies as periods; and while the next paragraph (542) asserts that the carbonic acid emitted from burning gas or candles "mixes,when cool, with the other heavy exhalations, and occupies the lower strata;" this is directly contradicted, and the facts correctly stated, in paragraph 549.

So also we are told, on page 196, that "the individual can *only* be sustained by an organic, and not by an inorganic food;" and yet in the next two lines it is contradictorily and correctly said, "It must not be supposed, however, that inorganic substances are useless in the nutrition of animals. On the contrary, the gases and minerals introduced with the water and nutritious decoctions, together with the ashy constituents and the salts, * * * all conduce to the necessities of animal existence." And again, "The substances required by animals for their sustenance and growth are of two kinds, the organic and the inorganic." These irreconcilable statements have probably escaped the author's attention, in the passage of the book through the press.

Though the opinion is not directly expressed, Dr. Bell seems inclined to the chemical theory of Liebig, on the subject of food, which Mr. Lewes, in his work on physiology, above mentioned, takes much exception to, and argues against, with no little acumen, especially in reference to the value of inorganic substances, which are to be considered in the light of food, as much as any of the organic matters used as such.

We are strongly tempted, at this point, to anticipate the course of our review of the works before us, and take up the discussion of the chemical theory of food, by quoting from the first volume of Mr. Lewes some of his remarks in answer to Liebig on this exceedingly interesting subject. But a desire to adhere to the plan marked out in this review, and the demand for the consideration of other important points, forbid the indulgence; but our readers will find, in the perusal of the *Physiology of Common Life*, a free and intelligent critique of the favourite idea of Liebig, enforced by quotations of facts, and other physiological writers, which carry great weight, and will be found of no little value in medical practice.

There are some other minor points and modes of expression and orthography (*e. g.*, Lichen *Icelandica*) to which a critic might take exception, but they are, perhaps, of insufficient consequence to justify further allusion, and some of the physiological facts presented are not fully up to the standard of the most recently obtained results.

The three works before us constitute a regular series (though of course unin-

tentional) of instruction on the subjects of which they treat. The first being elemental in character, is well adapted, under the guidance of a proper instructor, to serve as a text-book of the rudiments of physiology, for the use of schools, &c., while the second seems as if intended, as it certainly is well adapted, to lead the popular student through the next succeeding steps; and the third forms a fitting apex to the pyramid of instruction in living things.

2. To the productions of Mr. Lewes we, therefore, now turn with no little gratification and profit. Unknown to us before introduced by these works, we have been most agreeably disappointed, and even surprised, at the evidences of thorough familiarity with the subjects upon which he treats, the originality of his views, the exceedingly happy manner by which he takes his reader along with him, and the abundance of his illustrations and elucidations. We had little idea how, like an agreeable picture, the inner organism of animal life could be laid open to view, and how pleasant, and even amusing, a tale could be made of a description of its functions and sensations. The author is, in fact, a writer of great power; combining fervency of thought, and force and copiousness of language, with imagery partaking of the poetical, yet withal an experimentalist, and a practical and minute observer, of a too rare type.

To our mind, he is to animal physiology what Hugh Miller was to geology, an original and acute observer, and a vivacious expositor.

Though by no means the best we could select, as a specimen of his descriptive powers, we present the opening paragraphs of his first volume, from the chapter on "Hunger and Thirst."

"Hunger is one of the beneficent and terrible instincts. It is, indeed, the very fire of life, underlying all impulses to labour, and moving man to noble activities by its imperious demands. Look where we may, we see it as the motive power which sets the vast array of human machinery in action.

"It is hunger which brings these stalwart navies together in orderly gangs to cut paths through mountains, to throw bridges across rivers, to intersect the land with the great iron ways which bring city into daily communication with city. Hunger is the invisible overseer of the men who are erecting palaces, prison-houses, barracks, and villas. Hunger sits at the loom, which with stealthy power is weaving the wondrous fabrics of cotton and silk. Hunger labours at the furnace and the plough, coercing the native indolence of man with strenuous and incessant activity. Let food be abundant and easy of access, and civilization becomes impossible; so indissolubly dependent are our higher efforts on our lower impulses. Nothing but the necessities of food will force man to that labour which he hates, and will always avoid when he can. And although this seems obvious only when applied to the labouring classes, it is equally though less obviously true when applied to all other classes, for the money we all labour to gain is nothing but food, and the surplus of food, which will buy other men's labour.

"Hunger, although beneficent, is no less terrible. When its progress is unchecked, it becomes a devouring flame, destroying all that is most noble in man. Hunger is a stimulus to crime, no less than to honest labour. It wanders through dark alleys, whispering desperate thoughts into eager ears; and it maddens the shipwrecked crew till they cast away all shame, all pity, all desire of respect, and perpetrate deeds which cannot be mentioned without horror. Hunger subjugates the humanity in man, and makes the brute predominate. Impelled by this ferocious instinct, men have eaten their companions, and women have eaten their own children. Hunger has thus a twofold character; beside the picture of the activities it inspires, we must also contemplate the picture of the ferocities it evokes."

Again, when speaking of the "causes of hunger," the author thus demolishes a favourite popular theory: "The animal body is often compared with a steam-engine, of which the *food* is the *fuel* in the furnace, furnishing the motive power. As an illustration this may be acceptable enough; but, like many other illustrations, it is often accepted for a real analogy, a true expression of the facts. As an analogy its failure is conspicuous. No engine burns its *own substance* as fuel; its motive power is all derived from the coke consumed; when the coke is exhausted the engine stops. But every organism consumes its own body; it

does not burn food, but tissue. The fervid wheels of life were made out of food, and in their action motive power is evolved.

"The difference between the organism and the mechanism is this: the production of heat in the organism is not the *cause* of its activity but the *result* of it; whereas, in the mechanism, the activity originates in, and is sustained by, the heat. Remove the coals which generate the steam, and you immediately arrest the action of the mechanism; but long after all the food has disappeared, and become transformed into the solids and liquids of the living fabric, the organism continues to manifest all the powers which it manifested before."

Not the least interesting points of these volumes are the freedom and skill exhibited by their able author in combating the views of several distinguished physiologists, whose theories have found very general acceptance, derived from what appear to be exact and conclusive experiments. The chemical and physiological theories of Liebig, Marshall Hall, Flourens, Gall, Müller, Cuvier, Spallanzani, Leuwenhoek, and others, undergo, in these pages, various vigorous and keen, though perfectly fair and scientific criticism, showing manifest errors, and a setting to rights of the facts in more strict accordance with modern observations. The author is by no means a mere copyist. He is an original experimenter and thinker. His own microscope and knife open the pathways of investigation, and when he finds a fact opposed to the theories which have been laid down by others, he boldly proclaims it, and reasons therefrom with acumen and ingenuity, and in a style which never fails to interest, and often to captivate.

Where there is so much that is interesting, it is not easy to select any one portion of a volume without apparent injustice to all the rest. We must therefore content ourselves with confidently assuring our readers that the *Physiology of Common Life* will satisfy them, that its author is no ordinary writer or observer; and whether his views find general acceptance or not, he certainly presents and maintains them with remarkable clearness and force. Among the latest, so is he also among the most finished, of the now greatly enlarged number of physiological students, who, *con amore*, delight in delving into the hidden mysteries of nature, and like every true student of nature, he hesitates not to seek aid wherever he can find it, giving ample acknowledgment to all from whom he obtains it. His references are copious, and to our own Dalton and Draper as freely and as often as to any. It is a work destined, we believe, to make a decided mark upon the intelligence of the world, and is to be valued not more for the scientific information it conveys, than for the felicitous *popularization* of its style, whereby its facts are brought home to the comprehension of other readers and students than those of the medical profession.

3. The last work of the series before us, *Studies of Animal Life*, while marked by the same vivacity of style, and abounding in instruction, is yet very different from the work just noticed from the same hand. If we were asked to mention the characteristic most prominent in its pages, we should reply, "Enthusiasm." Though disclaiming the idea which some enthusiastic students have promulgated, that

"The proper study of mankind is *cells*;"

and regarding man as the noblest study, he yet finds under him some other problems which are not to be neglected. Man himself is imperfectly known, because the laws of universal life are imperfectly known. Man forms but the apex of the animal world, and he can only be understood thoroughly by first studying those minuter and obscurer forms, which, though they seldom attract attention, constitute the basis of the pyramid of which he is the apex, and must be conned and made familiar before his biological record can be fully unfolded and comprehended. In the glowing language of our author, "In the air we breathe, in the water we drink, in the earth we tread on, life is everywhere. Nature *lives:* every pore is bursting with life; every death is only a new birth, every grave a cradle! And of this we know so little, care so little! Around us, above us, beneath us, that great mystic drama of creation is being enacted, and we will not even consent to be spectators! Unless animals are obviously useful, or obviously hurtful to us, we disregard them. Yet they are not alien, but akin. The life that stirs within us stirs within them."

With the same design, in many respects, as the work of Dr. Bell before noticed, we are obliged to award to the latter the palm for regularity and system, and a more consecutive method of development of the subject in hand. In fact the variety of topics discussed in this little book of "Studies" of less than 150 pages, is too great to enable an ordinary reader to obtain from it all the satisfaction desired upon any one of them. From the examination of the parasitic animalcule found in the intestinal tube of the frog, which has neither head, nor tail, nor stomach, we are treated to a discussion on the "Fixity of Species," and a biographical sketch of M. le Baron Cuvier, "administrator, politician, academician, professor, dictator," whose simple, ardent, scientific youth, contrasted so widely with his ambitious, political, ceremonious age. Still the ardent lover of nature is traced on every page, and the book is a charming companion, for an evening hour, or a day of summer travel. It is freely illustrated by drawings of microscopic objects, and he tells us, "As a beginning, get a microscope. If you cannot borrow, boldly buy one. Few purchases will yield you so much pleasure; and, while you are about it, do, if possible, get a good one. Spend as little money as you can on accessory apparatus and expensive fitting, but get a good stand and good glasses. Having got your instrument, bear in mind these two important trifles—work by daylight, seldom or never by lamplight; and keep the unoccupied eye *open*. With these precautions you may work daily for hours without serious fatigue to the eye."

Of the numerous anecdotes recited by the author, we quote but one, and this to show the *contagiousness* of the spirit of the seeker of pleasure in the realms of nature.

"Very unintelligible this would be to the passers by, who generally cast contemptuous glances at us when they find we are not fishing, but only removing nothings into a glass jar. One day an Irish labourer stopped and asked me if I were fishing for salmon. I quietly answered 'Yes.' He drew near; I continued turning over the weed, occasionally dropping an invisible thing into the water. At last a large yellow-bellied Triton was dropped in. He begged to see it; and, seeing at the same time how alive the water was with tiny animals, became curious, and asked many questions. I went on with my work; his interest and curiosity increased; his questions multiplied; he volunteered assistance, and remained beside me till I prepared to go away, when he said seriously, 'Och! then, and its a fine thing to be able to name all God's creatures.' Contempt had given place to reverence; and so it would be with others, could they check the first rising of scorn at what they do not understand, and patiently learn what even a roadside pond has of nature's wonders." J. H. G.

ART. XXII.—*Operative Surgery, adapted to the Living and Dead Subject.* By C. F. MAUNDER, Part I. and II., complete, London, 1860–61, 12mo. pp. 318.

THIS work has been published, as the author states in the preface, to satisfy the demand which, while conducting a course of operations on the dead subject in Paris, he found to exist in the English Professional Library, for a concise, systematic, and illustrated guide to the operations applicable as well to the dead as to the living subject.

The volume he has compiled, with this view, is very concise, and it is, in some portions, profusely illustrated; but it is not by any means systematic, and while some few subjects are carefully and fully treated, by far the greater number receive a very insufficient notice.

Considerable experience in courses of operative surgery on the dead body has taught us that three subjects engross almost the whole attention of the teacher; these are, special amputations, the ligation of special arteries, and excision of the joints. These three subjects, and these alone, we have found treated of with proper care in the book before us; and upon all other subjects connected with operative surgery, we must pronounce it to be signally defective.

W. F. A.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On the Molecular Theory of Organization.* By T. Hughes Bennett, M.D., F.R.S.E.—Parodying the celebrated expression of Harvey, viz., *omne animal ex ovo*, it has been attempted to formularize the law of development by the expression *omnis cellula e cellulâ*, and to maintain "that we must not transfer the seat of real action to any point beyond the cell."[1] In the attempts which have been made to support this exclusive doctrine, and to give all the tissues and all vital properties a cell-origin, the great importance of the molecular element, it seems to me, has been strangely overlooked. It becomes important, therefore, to show that real action, both physical and vital, may be seated in minute particles, or molecules much smaller than cells, and that we must obtain a knowledge of such action in these molecules if we desire to comprehend the laws of organization. To this end I beg to direct attention:—

1st. To a description of the nature and mode of origin of organic molecules.

2d. To a demonstration of the fact that these molecules possess inherent powers or forces, and are present in all those tissues which manifest vital force.

3d. To a law which governs the combination, arrangement, and behaviour of these molecules during the development of organized tissue.

I. By a *molecule* is to be understood a minute body, seen under high magnifying powers in all organic fluids and textures, varying in size from the four-thousandth of an inch down to a scarcely visible point, which may be calculated at much less than the twenty-thousandth of an inch in diameter. Optically it is distinguished according to its size—the smallest presenting dark or light points as the focus is changed, and the larger exhibiting a dark or light centre, surrounded by a distinctly shadowed ring. These last are frequently distinguished by the name of *granules*. The ultimate molecule had never been reached even with the highest magnifying powers. In the same manner that the astronomer with his telescope resolves nebulæ into clusters of stars, and sees other nebulæ beyond them, so the histologist with his microscope magnifies molecules into granules, and sees further molecules come into view. The chemical composition of these molecules must vary infinitely; but I have been in the habit of classifying them into three groups, and referring them to—1. The albuminous; 2. The fatty; and 3. The mineral compounds. These constituents may be mingled together in various proportions so as to produce simple and compound molecules. In the vast majority of cases they are globular in shape; but they may be angular, square, and of various forms. They may differ in size, or be of tolerably uniform size, in the same liquid or substance. They may be regularly or irregu-

[1] Virchow, English Translation, p. 3.

larly diffused in the matter examined. Sometimes they are concentrated in particular places, and at others scattered in groups. Their colour is various. Most of the pigments in plants and animals are dependent on the formation of molecules, which in the human lung have been proved to be pure carbon, and in the tissues of plants and animals differently tinted kinds of fat or of wax.

These molecules may be formed in two different ways: 1. By precipitation in fluids; 2. By the disintegration of previously formed tissues. The former may be called *histogenetic* (ιστος and γενεσις, *generatio*), and the latter *histolytic* (ιστος and λυσις, *dissolutio*). They may be denominated molecules of formation and molecules of disintegration.

Histogenetic molecules are formed either from the union of two simple organic fluids, or from precipitations occurring in formative fluids, holding various substances in solution. Fourteen years ago I read to the Royal Society of Edinburgh a paper giving an account of the results obtained by a union of oil and liquid albumen, the two organic fluids from which molecular matter is most commonly derived. It was Dr. Ascherson, of Berlin, who first discovered the important fact that the mere contact of oil and fluid albumen caused the latter to coagulate in the form of a membrane, which he called the haptogen membrane, from ἅπτομαι, to come in contact. A more complete mixture of two such drops produces, as is well known, a white opaque fluid or emulsion, which in structure exactly resembles milk; that is to say, it consists of molecules composed of a drop of oil surrounded by a layer of membrane of coagulated albumen. Such compound molecules, possessing the property of endosmose, may therefore readily be produced artificially, and by trituration can be reduced in size so as to resemble the elementary molecules in chyle or in the yolk of an egg. If oil and albumen be introduced into the stomach and intestinal canal, they are always so reduced; and one of the objects of digestion would appear to be separating from the food, and rendering fluid its oil and albumen, so as to produce the chyle molecules which are ultimately transformed into blood. Indeed, everywhere in living organisms it may be observed that oil and albumen formed as secretions by plants, and entering the bodies of animals as food, either separately or united, constitute the chief origin of molecular formations.

Mr. Rainey has recently pointed out the condition which causes molecular mineral matter to assume the form of rounded nuclear bodies.[1] This condition is viscosity. If carbonate of lime be dissolved in water, the forms produced on its precipitation are crystalline; but if the fluid be glutinous, composed, for example, of fluid, gelatine, or gum, the forms produced are oval or globular. Precipitations made in this way, on slides of glass, closely resemble the appearances called nuclear or cellular in different stages of development. Mr. Rainey has further shown how starch-granules are produced in the juices of vegetables by the endosmose of gum into a cell containing a solution of dextrine.[2] In the same manner that the contact of oil and albumen produces oleo-albuminous molecules, so does the contact of gum and dextrine precipitate starch-molecules. In this manner we can comprehend how the mixture of various organic fluids gives rise to particles of different kinds.

Histolytic molecules are the result of the transformation and disintegration of fluid and solid substances by chemical or mechanical action. They are generally larger in size than histogenetic molecules, are more purely fatty, and from being associated with the *débris* of broken-down texture may, in most instances, readily be distinguished. Thus, in the breaking up of cells and of muscles when they become fatty, or in the putrefaction of vegetable or animal matters, these may be seen to soften, lose their peculiar structure, break up, and ultimately be reduced to a molecular condition.

We shall subsequently see that these two kinds of molecules are constantly changing places; or, in other words, molecular matter formed from the process of disintegration may, when placed under peculiar circumstances, become the

[1] On the Mode of Formation of Shells of Animals, of Bone, and of several other Structures, by a Process of Molecular Coalescence, etc. By George Rainey, M.R.C.S. London, 1858.

[2] Microscopical Journal, 1859.

basis of matter which undergoes development. In nature, the breaking down of one substance is the necessary step to the formation of another, and the histolytic or disintegrative molecules of one period become the histogenetic or formative molecules of another. This fact constitutes the basis of the law which I shall subsequently seek to establish.

II. These molecules are governed by forces, which induce amongst them a variety of movements, and cause them to combine in definite ways. This force, which we may call *molecular force*, is altogether independent of cell, nucleus, or other form of structure.

1st. There are the well-known molecular movements described by Robert Brown. These vibratile, circular, serpentine, or irregular motions may be observed whenever molecules are suspended in fluids of certain densities, but are too well known to require notice here. They occur altogether independent of organized structures, and must be regarded as in their nature purely physical.

2d. The peculiar movements observed in the interior of cells, vegetable or animal, and during the putrefaction of organic matter. The former are seen in the large vegetable cells of the Chara, Valisneria, and Tradescantia, amongst plants; and those of chyle, the yelk of the egg, and of the salivary cell amongst animals. I have frequently watched the formation of the latter in putrid fluids. A scum, composed of molecules, collects on the surface; gradually several of these unite in minute filaments, more or less long, which assume vibratile or serpentine movements. They are then called *vibriones*. It has been much disputed whether this class of molecular motions be physical or vital.

3d. The movements, which are unquestionably vital, that occur in the molecules of the yelk, on the entrance into the ovum of the spermatozoid. Here it cannot be maintained that the results are purely physical, because in different ova we see such widely-varying effects from apparently the same cause. Neither can it be attributed to any direct influence of the cell or of its nucleus—the germinal vesicle. For example, an egg is fully maturated in the female organs of generation, and would prove abortive if a spermatozoid did not find its way through the zona-pellucida and get amongst the molecules of the yelk. As soon as it does so, the apparently purposeless Brunonian movements receive a new impulse and direction. Both spermatozoid and germinal vesicle are dissolved amongst them; and that wonderful phenomenon of the division of the yelk takes place, not by cleavage or other action of the cell-wall or nucleus, but by the separation of the mass into two masses instead of one. This may be compared to what is observable in a dense crowd of men, called upon to pass over to the right or left hand in order to settle any disputed question by a majority. At first unusual confusion is communicated to the whole; some hurry in one direction, others in another; but, after a time, there is seen at the margins, where the crowd is least dense, a clear space, which gradually approaches the centre, and at length bisecting the whole, produces a complete segregation of the crowd into two portions. So with the molecules of the yelk in the egg after impregnation; their movements are directed by conditions which did not previously exist, and a stimulus is imparted to them which causes the peculiar result. It is the division and subdivision of the yelk, wholly or in part, which produce the germinal mass out of which the embryo is formed, and this not by any direct influence of the cell or nucleus, but in consequence of a power inherent in the molecules themselves, which is communicated to them for a specific purpose.

4th. The peculiar movements so well described by Brücke, Von Wittich, Harless, and especially by Lister, in the pigment-cells of the frog's skin,[1] and which occasion the sudden change of colour in the cameleon, in fishes, and numerous other animals. The black pigment molecules may be diffused throughout the cell or concentrated in a mass, and all kinds of intermediate gradations may exist between diffusion and concentration. The change in colour is owing to these alterations in the molecules, the tint being light when they are concentrated, and dark when they are diffused. Mr. Lister ascertained by experiment that their concentration is caused by exposure to light, by death of the animal,

[1] On the Cutaneous Pigmentary System of the Frog; Philosophical Transactions, 1858.

and by sudden section of the nerve going to the skin; while darkness and irritation of the nerve or skin causes diffusion. Sudden amputation of a limb produced at first diffusion, followed by the concentration of death. These movements of the pigment molecules are peculiarly vital, and altogether independent of the cell-wall or nucleus. The cell-wall is stationary, and acts only as a sac or investing membrane around the moving particles, while the concentration of these about the nucleus is purely accidental, and frequently occurs in other parts of the cell. I have seen these molecules myself, as Mr. Lister describes them, streaming out to, and returning from, the circumference under the influence of the stimuli referred to, where no cell nor nuclear action could be thought of.

5th. There are many other kinds of movements which are evidently independent of cells—for example, those of cilia and of spermatozoids. The former are outside cells, and the latter only move when they are liberated from cells. The contractile fibrillæ of muscle are evidently not dependent for their inherent power on cells or other form of structure, but on the square-shaped molecules of which its substance is composed. All these phenomena, therefore, are connected with the molecules themselves; the force occasioning them is a molecular force, and has nothing to do with pre-existing cells, or supposed germinal centres, as some have imagined.

Again, the power of combination between these molecules, which, under peculiar conditions, not only move, but so move as to advance towards and press upon each other, that they at length unite and produce higher forms, must also be attributed to a molecular force operating in obedience to fixed laws. Thus it was demonstrated by Newton, that in a sphere the total attraction resulting from the particular attraction of all its component parts is, as regards any body drawn towards it, the same as if they had been concentrated at the centre. Hence minute spherical particles, as so many gravitating points, will be drawn towards each other with a force varying inversely as the squares of the distances between their respective centres. I may here refer to the able descriptions of Mr. Rainey,[1] as to the physical laws regulating the formation and disintegration of bodies by molecular attraction and repulsion as well as to the effects of molecular superposition, showing that the same physical power which leads to the formation of these artificial bodies, when long continued, causes their disintegration and destruction. All these changes occur slowly, and require time; but their contemplation, when regarded as purely physical phenomena, must strike us with surprise, as being closely allied to all our conceptions of the progress of life itself.

In making use of the expression *life* and *vital action*, I am only using terms to indicate phenomena which, in the present state of science, cannot be accounted for by the ordinary laws of physics. Or it might be said that certain actions are directed and governed by conditions which are as yet undetermined, but which, as they only occur in organic, as distinguished from inorganic bodies, constitute vital actions. Not that an organized body is independent of physical forces, but that certain directions are communicated to them, which, as invariably resulting in specific forms or properties, make up the sum of what we call vitality.

Hence, although we see molecules combining in the forms of crystals and nucleated spherules, inasmuch as we have discovered the physical conditions on which they depend, and can produce them artificially, we have no difficulty in classifying these amongst purely physical phenomena, even when they occur in the interior of animals. But when other molecules unite to form nuclei, cells, and fibres, and these arrange themselves into tissues and organs to produce plants and animals, we are ignorant of the conditions by which these results are brought about—we cannot imitate them artificially, and are content to call them vital. But the fact I am anxious to point out is this, that so far as observation and research have enabled us to investigate this difficult matter, it appears that the formations and disintegrations of vegetables and animals, as well as the peculiar properties they exhibit, are essentially connected with the molecular

[1] *Op. cit.* See also papers in the Microscopical Journal, 1860.

element. Thus, when we investigate the functions of plants and animals—for example, generation, nutrition, secretion, motion, and sensation—we find them all necessarily dependent on the permanent existence and constant formation of molecules.

Thus generation, both in plants and animals, is accomplished by the union of certain molecular particles called the male and female elements of reproduction. Amongst the Protophyta the conjugation of two cells enables their contents, or the endochrome, to mix together. This endochrome is a mass of coloured molecules, and the union of two such masses constitutes the essential part of the generative act. In the Cryptogamia, a vibratile antheroid particle enters a germ-cell, and finds this last filled with a mass of molecules, which, on receiving the stimulus it imparts, assumes the power of growth. It is the same amongst the Phanerogamia, when the germ-cell is impregnated by the pollen tube. In all these cases it is necessary to remember that the protoplasm is a mass of molecules; that a spore is another mass of molecules; that sporules are molecules; that antherozoids are only molecules with vibratile appendages; and that the so-called germinal matter of the ovule is also nothing but a mass of molecules. Cell-forms are subsequent processes, and once produced may multiply endogenously, by gemmation or cleavage; all that is here contended for is, that the primary form is molecular, and that the force-producing action in it is a molecular force.

In animals, as in vegetables, every primary act of generation is brought about by the agency of molecules. The *Protozoa* entirely consist of mere molecular gelatiniform masses, in which it has never been pretended that a cell-wall or central cell exists. And yet such masses have the power of independent motion, and of multiplying by gemmation. Considerable discussion has occurred as to whether, among *Infusoria*, there is a union of sexes or a conjugation similar to what occurs among the *Protophyta;* but, in either case, it is by molecular fusion that the end is accomplished. In the higher classes of animals there are male elements, consisting of molecules, generally with, but sometimes destitute of, vibratile filaments, and female elements, composed of the yolk within the ovum, containing a germinal vesicle or included cell. Both spermatozoid and germinal vesicle are dissolved in the molecules of the yolk, which then, either wholly or in part, by successive divisions and transformations, constitute a germinal mass out of which the embryo is formed. Here, as in plants, it is necessary to remember that the spermatozoids, the yolk, and the germinal mass, are all composed of molecules, and that these, combining together, form the nuclei, cells, fibres, and membranes, which build up the tissues and organs of the organism. It is not from either the male or the female element that the embryo is formed. The supporters of an exclusive cell-doctrine have endeavoured to show that there is always a direct descent either from the wall of the ovum or from the germinal vesicle as its nucleus. Thus some consider that the vitelline membrane sends in partitions to divide the yolk mechanically. Others have formed the idea that the germinal vesicle bursts, and that its included granules constitute the germs of those cells which subsequently form in the germinal mass. Others, again, suppose that on impregnation the germinal vesicle divides first, and that the molecules of the yolk are attracted round the two centres so formed. But numerous observations have satisfied me that both spermatozoid and germinal vesicles are simply dissolved among the molecules of the yolk, from the substance of which, stimulated and modified by the mixture so occasioned, the embryo is formed; a view which has further the merit of explaining what is known of the qualities of both parents observable in the offspring. I am only acquainted with one exception to this general law—namely, the development of *Pyrosoma*, recently described by Mr. Huxley, the description of which, however, is incomplete. The truth appears to be, that in an analogous manner to that in which the pigment molecules of the skin are stimulated by the access of light to enter into certain vital combinations with one another, so are the molecules of the yolk stimulated by the access of the spermatozoid to produce those other vital combinations that result in a new being.

[1] Annals of Natural History, Jan. 1860, p. 35.

The essential action is not so much connected, as has hitherto been supposed, with the cell-wall or nucleus as with the molecular element of the ovum.

With regard to nutrition; food and all assimilable material must be reduced, in the first instance, to the molecular form, while the fluid from which the blood is prepared, viz., chyle, is essentially molecular. Most of the secretions originate in the effusion of a fluid into the gland-follicle, which becomes molecular, and gives rise to cell-formation. In muscle, the power of contractility is inherently associated with the ultimate molecules of which the fasciculus is composed; and, lastly, the gray matter of the sensory ganglia, and of the brain, which furnishes the conditions necessary for the exercise of secretion, and of even intellect itself, is associated with layers of molecules which are unquestionably active in producing the various modifications of nervous force. These molecules are constant and permanent as an integral part of these tissues, as much as cells or fibres are essential parts of others, and their function is not transitory, but essential to the organs to which they belong.

All these facts point to the conclusion that vital action, so far from being exclusively seated in cells, is also intimately associated with the elementary molecules of the organism.

III. This leads me, in the third place, to an enunciation of the molecular law of growth, which a study of the numerous facts previously referred to had induced me to frame, viz:—*That the development and growth of organic tissues are primarily owing to the successive formation of histogenetic and histolytic molecules.* We have already seen that development and growth in animals originate in the molecules of the yolk of the egg, or of a germinal molecular mass formed from it. From numerous careful researches recognized by scientific men as giving a correct account of the development of various animals and textures, it would appear that the first form is molecular; that the molecules unite to produce nuclei and cells; that these become disintegrated to produce a secondary mass of molecules; that these again unite to form secondary nuclei and cells; and that the same process is repeated more or less often in various developments, until the animal or tissue is formed. This constitutes the successive histogenetic and histolytic molecules observable in the process of growth; the former building up, to a certain extent, and the product disintegrating to produce the latter, which, after a time, again rearranges itself, and becomes histogenetic to form cells or tissues, which, in their turn, break down and become histolytic. In short, not only development, but growth and secretion, absorption and excretion, are only different names given to histogenetic and histolytic processes, and these are brought about by formative and disintegrative molecules. As illustrations of this law, I may refer to the development of *Ascarix mystax*, as described by Nelson[1], and of the process of nutrition in the human body.

In this, and a vast number of similar observations, it must be evident that a certain series of molecular transformations is necessary for the one which follows it. Thereby is produced a continual elaboration of matter—a constant chemical and morphological series of changes—the exact number and order of which, in the production of organic forms, only require time and perseverance to discover. Doubtless, various conditions, dynamical, chemical, and vital, must co-operate in producing the result, and they must all influence molecular as well as every other kind of combination. Such considerations and facts must convince us of the error of endeavouring to place the source of special vital action in any particular form or arrangement of organic matter, whether fibre, cell, nucleus, or molecule. Each and all of these elements have their vital endowments, which reoperate on the others. But, inasmuch as the molecular element is the first as well as the last form which organized matter assumes, it must constitute the principal foundation of organization itself.

It is not my object, in directing attention to a molecular theory of organization, to interfere in any way with the well-observed facts on which physiologists have based what has been called the cell-theory of growth. True, this last will require modification, in so far as unknown processes of growth have been hypothetically ascribed to the direct metamorphosis of cell elements. But a cell once

[1] Phil. Trans., 1850, pl. xxviii. xxix. figs. 59, 68, 70, 78.

formed may produce other cells by buds, by division, or by proliferation, without a new act of generation, in the same manner that many plants and animals do, and this fact comprehends most of the admitted observations having reference to the cell-doctrine. The molecular, therefore, is in no way opposed to a true cell-theory of growth, but constitutes a wider generalization and a broader basis for its operations. Neither does it give any countenance to the doctrines of equivocal or spontaneous generation. It is not a fortuitous concourse of molecules that can give rise to a plant or animal, but only such a molecular mass as descends from parents, and receives the appropriate stimulus to act in certain directions.

In conclusion, the theory I have endeavoured to establish on histological and physiological grounds, is fully supported by all the known facts of disease and of morbid growths, which further serve to show that pathology, so far from being cellular, is, in truth, molecular.—*Proceedings of Royal Society of Edinburgh*, April 1, 1861.

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2. *Pulmonary Osmosis.*—The frequent coincidence of diabetes and pulmonary tubercles has led Mandl to perform experiments on the influence of solutions of sugar and some other sweet substances (cane sugar, glucose, sugar of milk, glycerine, and mannite) on various species of aquatic animals, as infusoria, annelida, crustacea, water-insects, fishes, and batrachia. The results are: 1. That all aquatic animals perish in solutions of sweet substances; there is, however, a difference as to the time of death, depending on the degree of concentration of the solution, on the quality of the substance contained in the solution, and on the species of animal. Thus, the infusoria died almost instantaneously in solutions of 20 per cent. of cane sugar, glucose, glycerine, and mannite, but lived about five minutes in a concentrated solution of sugar of milk; they died after six or eight minutes in a solution of 4 per cent. of mannite, while they lived three times as long in a solution of equal strength of cane sugar. Some fishes (12 to 15 centimetres long) perished in a solution of glycerine of 10 per cent. after about forty minutes; in a solution of cane sugar of the same strength only at the end of four or five hours. 2. That the death is not caused by a chemical action of these substances, or by fermentation, or absence of air, or by the viscosity of the fluid, but only by the osmotic action (endosmosis and exosmosis) exercised by the solutions of the sweet substances. 3. That this osmotic process takes place through the permeable membranes, and especially through those of the respiratory organs. The not fermentable sweet substances possess a greater osmotic power, and act also more rapidly on the life of the animals. 4. Experiments made with animal membranes, to demonstrate which of the elements of the blood pass over into the solution of sugar, have shown that the first to pass over is water charged with the salts of the serum, after which follows albumen, the colouring matter being the last. The passage of the albuminous substances is retarded by greater concentration of the osmogenous substance. Mandl promises to communicate in another paper the results of his injections of solutions of sugar into the respiratory organs, with regard to the formation of tubercles of local nature, in opposition to the tubercles resulting from the tubercular diathesis. In the present essay, the author points out how the osmogenous power of sugar explains various physiological and pathological phenomena, as the sensation of thirst after the ingestion of sugar; the antiseptic power of sugar; the digestive property of small quantities of sugar, causing exosmosis of gastric juice; the absorbing power of large quantities of sugar in dropsies; the great thirst of diabetic individuals; the impossibility of serous effusions in such patients. To the great osmotic action of the glycerine, the author ascribes also the influence of this substance as a local remedy.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Comp. Rend.*, vol. i. 1860, and *Arch. Gén. de la Méd.*, vol. ii.

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3. *Death by Submersion.*—Dr. Beau has performed on dogs several series of experiments, to elucidate the cause of death by submersion, especially with the view to explain the fact, that the air-passages of drowned animals contain only a very small quantity of fluid. The inference is, that the immersion of the

natural orifices of respiration causes, "by sympathetic or reflex action, spasmodic occlusion of the sphincters or orifices of respiration, and stopping of respiratory movements." The death from drowning, the author argues, is therefore analogous to that from tetanic affection of the muscles of respiration. The two principal experiments are, that of two animals, in whose opened trachea a canula has been inserted; the lungs of the one which had been entirely submerged under water contain only the usual small quantity of frothy fluid; while the air-passages of the other, whose head had been kept out of the water, are filled with that fluid.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Comp. Rend.*, vol. i. 1860.

4. *Modes of Death from Chloroform.*—Much difference of opinion exists as to the nature of the death of those in whom chloroform has proved fatal. Whether death is due to syncope or to apnœa; to paralysis of the circulation or of respiration, or whether either may be the cause of death, is still a mooted question. Dr. A. E. SANSOM states, as the result of his investigation, that either of these may be the cause of death. He observes (*Lancet*, Aug. 24, 1861) "all the vital functions in etherization are depressed by an altered blood. What link is first snapped asunder in fatal cases is determined by various causes. In animals respiration fails first because the automatic power of their hearts enables its movements to persist. In man, circulation usually first fails because the heart is the most susceptible of the incomplete stimulus. In animals death occurs by asphyxia, and death begins in the brain. In man, death occurs by syncope or by asphyxia, and death begins in the brain, in the heart, or in the lungs. Practical men may learn a useful caution from this; for it has been said, that in cases of danger the pulse is to be disregarded, and the embarrassment of the breathing taken as the critical sign. If I may be permitted to express dogmatically what I have learnt by experience, I would say: Rely almost wholly on the pulse. Disregard dyspnœa if the pulse be good; but if the pulse show signs of failing, withdraw the chloroform, and consider the condition a condition of danger.

MATERIA MEDICA AND PHARMACY.

5. *Rules for the Administration of Chloroform.*—Dr. W. MARCET has published (*Med. Times and Gaz.*, July 20, 1861) some interesting practical observations on the action of chloroform.

"When chloroform," he observes, "is inhaled, and consequently brought into contact with the air-cells of the lungs, it passes rapidly into the blood, by means of which it is carried to the brain. If the administration of the anæsthetic agent be suspended, the chloroform will be eliminated from the body by the respiration, each inspiration displacing most of the vapour contained in the blood exposed by the lungs to the action of air during that inspiration. The elimination from the blood of any very volatile substance possessed of a stable chemical composition may be considered, as a rule, to take place through the lungs. This might have been anticipated by a consideration of the displacement of the carbonic acid of the blood by the air inspired, and has been placed beyond doubt by a well known beautiful experiment of Claude Bernard, where an aqueous solution of sulphuretted hydrogen being injected into the blood of a dog, the animal in the course of one or two minutes expires the whole of the poisonous gas. Messrs. Lallemand, Perrin, and Duroy have shown experimentally that this law is applicable to chloroform, and consequently there is not the slightest doubt that when blood contains chloroform it is removed therefrom by means of respiration.

"If the air inspired be pure, the displacement of chloroform from the blood in the lungs will be very great; if this air should contain chloroform the displacement will be less, just as when air containing a large proportion of carbonic acid is breathed, the removal of the carbonic acid of the blood is checked. When a

patient begins to inhale chloroform, a portion is absorbed by the blood, the remaining is expired; but shortly afterwards, in addition to the expiration of that part of the chloroform, which has not been taken up by the blood, a certain quantity of that which has been absorbed is also ejected, being displaced by the air mixed with the chloroform inhaled. At this stage, however, there is still an accumulation of the anæsthetic agent in the blood, more being taken into the circulation than given out; gradually complete insensibility is produced, and the handkerchief is removed from before the face of the patient; he now begins ridding himself rapidly of the chloroform, and recovers consciousness, unless more of the anæsthetic agent be exhibited. By the careful administration of chloroform the state of insensibility may be kept up for a considerable length of time; during this period it is obvious that the accumulation of the vapour in the blood no longer takes place, otherwise it would invariably produce death, there must consequently be an equilibrium between the quantity of chloroform absorbed, and that which is displaced and eliminated by the process of respiration. If, during this stage of insensibility from any cause whatever, the power of absorption of the blood for chloroform be suddenly increased, or its property of giving it out to the air inspired be diminished, then death will take place from an accumulation of the vapour in the blood. It is difficult to imagine that the power of blood of absorbing the substance under consideration should be suddenly increased; but there is a very simple cause impairing its elimination from the blood, viz., the administration of the chloroform vapour in too concentrated a condition. Just as an excess of carbonic acid in the air prevents or interferes with the elimination of that contained in the blood, so must an excess of chloroform in the air prevent or interfere with the exit of chloroform already existing in the blood; therefore, the blood goes on taking up chloroform, and giving out less than a quantity equal to that absorbed, at the same time the evil may be increased by a few deep inspirations taken unconsciously, although apparently with the view of ejecting the poison, and life is suddenly extinguished."

These observations lead him to the following conclusions:—

"1. That chloroform must be administered cautiously, and its effects watched with particular attention, if, although the vapour be freely inhaled, the patient does not become insensible within the usual time.

"2. That in every case where chloroform is administered, as soon as the state of insensibility is obtained, the vapour must be exhibited diluted as much as possible with pure air; and air free from the anæsthetic agent ought to be allowed frequently into the lungs to remove the excess of the vapour present in the blood.

"3. That during the administration of chloroform great attention should be paid to the state of the respiration, which ought to guide the exhibition of the anæsthetic agent still more than the condition of the pulse. If the inspirations become less deep and respiration appear failing, air free from chloroform ought to be immediately allowed into the lungs, not only because this state of the respiration is an indication of there being an overdose of chloroform in the blood, but also because the diminished respiration is in itself a cause of danger by preventing the blood from ridding itself of the chloroform it contains.

"4. That when a patient has sunk under the effects of poisoning by chloroform the only means of restoring animation is by artificial respiration, adopting such method as is best calculated to introduce as much air as possible into the lungs in order to remove the poison from the blood, at the same time stimulating the action of the heart.

"It is due to Messrs. Lallemand, Perrin, and Duroy to state that they have already called attention to the importance of looking closely to the respiration during the administration of chloroform; but these gentlemen have overlooked the fact that the presence of an excessive proportion of chloroform in the air inspired must act more or less as an obstacle to the elimination of the vapour which has already been absorbed—a circumstance which ought to be taken into consideration on every occasion where chloroform is exhibited."

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6. *Internal Administration of Chloroform.* By M. Bonnet and Dr. Debout.—In consequence of the difference in density between chloroform and water, it is rather difficult to administer the former drug as an internal remedy,

because it falls to the bottom of the bottle and can only be suspended by continual shaking. M. Bonnet has therefore proposed to employ glycerine as a medium for the administration of chloroform. Equal parts of chloroform and glycerine are poured into a mortar, and the mixture is rubbed up gently until no more drops of chloroform can be perceived; then the distilled water or other adjuvant is added, and a very limpid mixture is obtained, not exhibiting the slightest trace of chloroform in the free state. The glycerine should be quite pure—that is to say, it ought to be carefully freed from organic matters, fatty volatile acids, a little sulphuric acid, and certain salts, with which it is often adulterated in commerce. Dr. Debout approves the suggestion of M. Bonnet as to the mixture of chloroform with glycerine, but he recommends that the proportions of the two should be different. Dr. Debout's formula consists of thirty grammes of glycerine and two grammes of chloroform, a teaspoonful for a dose in a glass of water. The dose of chloroform thus taken amounts to twelve drops.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Bull. Gén. de Thérap.*, April 15, 1861.

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7. *On the Operation of Aniline upon the Animal Body.*—Dr. SCHUCHARDT made a series of experiments to prove the effects of aniline upon animals. Aniline is an oily liquid, of powerfully basic properties, obtained by the distillation of indigo, and the name aniline is derived from the specific name of indigo-plant (*Indigofera anil*). The physiological properties of aniline are but little known, but the researches of Dr. Schuchardt prove that it is a poisonous substance. The results at which he arrived are the following: Aniline may act injuriously on the animal organism, and in large doses may even cause death. Frogs introduced into a weak solution containing aniline died in periods varying from a quarter of an hour to two hours and a half, and death was also caused by the introduction of aniline into the mouth or into a wound in the back. Rabbits were also poisoned by this substance, a small animal being killed by fifty drops in six hours and a quarter, and a larger one by one hundred drops in four hours. In all the animals experimented upon, violent clonic and tonic spasms ensued after the application of the aniline, and continued almost uninterruptedly until death. There was also loss of sensibility, commencing at the lower extremities and extending to the upper, and the temperature of the body was also reduced. Wherever the aniline was applied locally, as in a wound of the back, on the stomach, on the posterior part of the tongue, or on the conjunctiva, appearances of irritation were observed, which are probably connected with the power possessed by aniline of coagulating albumen. The aniline was never detected in the urine, and it is probable that this substance is eliminated from the body rather by the organs of respiration than by the kidneys.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Archiv für Path. Anat. und Physiologie*, 1861.

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8. *On the Physiological Action of Santonine and of the Seeds of Artemisia Contra.*—Santonine is an organic principle obtained from the *Artemisia contra*, and possesses vermifuge properties, but its operation has only recently been minutely studied. Dr. EDMUND ROSE, of Berlin, has taken it himself, in the dose of a gramme (about fifteen grains), without injury. Its taste is bitter, very disagreeable, and persistent, and it is slightly diuretic, but rather confines the bowels. It produces a peculiar and indescribable narcotic effect, and causes a species of colour-blindness, tinging all the objects seen with a yellow hue. The essential oil of *Artemisia contra* is poisonous; it kills rabbits in the dose of about two grammes, producing convulsions which descend from the head to the lower extremities, followed by ascending paralysis. This oil is not vermifuge, for in medicine and medicinal doses it is absorbed in the stomach and the upper part of the small intestines, and does not reach the lumbrici, which generally exist lower down; in larger doses it is poisonous. Santonine, on the other hand, is a good vermifuge; it is innocuous, and traverses the whole of the digestive canal, and is found in large quantity in the fecal matters, for it is almost insoluble in water, and a small part only is transformed into soluble santonate of soda, after contact with the bile, and it is santonine which most rapidly de-

stroys the worms. Santonate of soda ought not to be substituted for santonine, as has sometimes been recommended, for it possesses no superiority over the latter, and is besides of a more disagreeable taste, and is poisonous in large doses.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Archiv für Path. Anat. und Physiologie, &c.*, 1860.

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9. *Therapeutical Employment of the Sulphate of Alumina and of the Sulphate of Alumina and Zinc.*—The sulphate of alumina (which is not to be confounded with the common alum, a double sulphate of alumina and potash) was introduced into medical practice by Dr. Homolle. It contains an excess of acid, a small portion of iron, and a little of the double sulphate; and in order to neutralize the free acid, which may burn the linen of the patients and injure the steel instruments of the surgeon, M. Homolle has proposed to add oxide of zinc, thus forming a double sulphate of alumina and zinc. This latter has a more energetic action on heteromorphous tissues than the simple sulphate, and M. Homolle prefers it when it is desirable to apply it to a deep portion of altered mucous tissue, or to destroy an accidental growth. The affections in which it has been successfully employed are inflammations of the tonsils and pharynx, hypertrophy of the tonsils, polypus of the nasal fossæ, ingrown finger-nail, scrofulous ulcers, nævi and vascular growths, inflammatory affections of the neck of the uterus, displacements of that organ, and lastly, cancerous ulcers. In many cases where the tonsils were so much hypertrophied as apparently to require excision, M. Homolle has observed the chronical enlargement to yield to the daily use of the saturated solution of the simple sulphate of alumina. Membranous diphtheric sore-throat, before the larynx was attacked, yielding to the same application, and in a case of polypus of the nasal fossæ, where the morbid growth had sprouted out several times after removal, the patient was finally cured by the same agent. Scrofulous ulcers, touched every day with the sulphate of alumina, began to assume a healthy appearance, their fungous growths being reduced and the cicatrization being promoted; and hypertrophic engorgement of the neck of the uterus, and erythematous, granular, or ulcerous inflammation of its lips, was rapidly improved or cured by the direct application of the double sulphate with the addition of injections of the same salt. In its direct action it favours the exfoliation of the morbid tissue in successive layers; it diminishes the ichorous discharge of the ulcerated surface, destroys the smell, and causes the secretion to resemble healthy pus, and diminishes or completely removes the lancinating pains which are peculiar to cancer. M. Homolle's conclusions, in reference to the use of the sulphate of alumina and the double sulphate of alumina and zinc, are the following:—

1. These salts, used externally, may be ranked as modifying agents, occupying a place between catheretics and caustics. 2. They are particularly useful in inflammations of the tonsils and pharynx, and in the affections of the neck of the uterus; and 3. They possess, moreover, a special remedial power over cancerous ulcers, the progress of which they retard or modify, acting in this respect at once as caustics, disinfectants, and hæmostatics. They often succeed better than narcotics in relieving the pain peculiar to cancer, and they retard the development of the attendant cachexia.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Bull. Gén. de Thérap.*, March 30, 1861.

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10. *Employment of Cajeput Oil.*—M. Delvaux, after giving a historical statement of the therapeutical employment of this substance, next furnishes an account of his own experience of its use when combined with passive substances, admitting of its properties being readily judged of. He has found it of great use in advanced and painful dental caries. A drop or two is inserted into the cavity and retained there by some cotton which had been dipped in the oil. After some minutes a sense of cold and numbness ensues, and the pain rarely resists. Essential dyspepsia, accompanied by intestinal flatuosity is treated with success by the oil. A tablespoonful of a mixture of two or three parts of the oil with 160 parts of mucilage and 30 of syrup may be given every hour, or a pill containing half a grain of the oil mixed up with magnesia may be substituted. The meteorism met with in severe diseases may be combated by an em-

brocation formed of equal parts of olive and cajeput oils. In algid cholera the oil is quite useless, but this is not the case in cholerine, or the disturbance of the digestive organs which so often precedes the cholera. A great number of such cases, when the pulse has been small, feeble, and slow, have been treated advantageously by the following mixture, aided by low diet and tepid drinks: R.—Mucilage 140, cajeput 3, syrup 30 parts. A tablespoonful, first every quarter and then every half hour. It is usefully employed for the expulsion of ascarides, either as an electuary made with honey (1, 2, or 3 parts to 60), or as an enema, one part being mixed with 30 of distilled water by means of yelk of egg. In chronic affections of the respiratory organs, as laryngitis, bronchitis, slow phthisis, accompanied by asthenia, the cajeput leads to a diminution of the dyspnœa and cough, and an easier expectoration while the appetite returns. It is of use in nervous palpitation. In two cases it was given in doses of from 15 to 45 grains in the 24 hours. In chronic vesical catarrh, the oil without curing the disease gives rise to great amendment by facilitating the emission of urine and diminishing the production of mucosities. As to its employment in rheumatism, M. Delvaux has always found it without effect while acute inflammatory action has been present. In the chronic articular rheumatism, when the pain and swelling are slight and the colour of the skin is normal, it is always useful, employed internally and by friction; but when the affection is attended with much swelling, or by deformity, or tophus, it is of no avail. In chronic myorheumatism and endorheumatism it is always of great service; while in epicranial rheumatism, in which the fibro-muscular layer of the cranium is affected, it has often effected a complete cure after numerous other remedies have failed. The same good effects have attended its employment in chronic pleurodynia and lumbago. For the muscular rheumatisms observed in those who have been exposed to the fatigue of prolonged voyages or residence in hot and humid climates, which are attended with emaciation, and in which digestion has become difficult, painful, or even impossible, in consequence of the fibro-muscular portions of the digestive canal having become involved in the rheumatism, cajeput proves a true specific, the pain disappearing under its influence and the patient recovering his health. In various *affections of the skin* this remedy is very efficacious, as acne rosacea, pityriasis, and psoriasis. In acne rosacea the oil must be applied over the whole diseased surface by means of a pencil three times a day, placing the patient in the horizontal posture lest the vaporization irritate the eyes. The epidermis scales off and the close network of vessels becomes atrophied, so that in persons with fresh complexions the skin to which the oil has been applied becomes whiter than that of the rest of the face. Furfuraceous desquamations of the scalp, eyelids, and beard, may be successfully treated by ointments containing the oil, and patches of psoriasis may be treated by imbibing them with the oil. This soon excites sharp inflammation and suppuration of the dermis, which excites a modifying influence upon the skin. The oil must then be replaced by soothing topical applications. Cajeput prevents the fall of the hairs, and favours their reproduction in alopecia when there is atony of the dermis or bulbs. It has proved especially useful in cases in which the eyelashes have fallen after having been dyed. In the painful atony of a joint with tumefaction and œdema met with after *sprain*, especially when the starch bandage has been used, frictions with embrocations of olive and cajeput oils soon re-establish the vigour of the part.—*Med. Times and Gaz.*, Aug. 17, 1861, from *Presse Médicale Belge*, 1861, Nos. 12, 13, 17, 23, 25, 30.

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11. *Preparation of a Stearate of Iron in the Treatment of Phagedenic Chancres.*—For some months M. RICORD has employed an ointment and an adhesive plaster of stearate of iron, which appears to be a valuable agent for dressing chancres which are complicated with phagedena. This new therapeutical agent was employed for the first time on a patient who had been the subject of experiments in syphilization, and who had, when he came under M. Ricord's care, his thighs covered with phagedenic ulcerations, which had been treated without success by several able practitioners. M. Ricord conceived the idea of making use of an adhesive plaster of stearate of iron, with which he dressed the ulcerations on the right side, and dressings, by way of comparison,

were applied on the left side with adhesive plasters of coal-tar. In a short time the phagedenic ulcerations on the right side were completely cicatrized, and in consequence of this remarkable result, the coal-tar plaster was replaced by one of stearate of iron, which effected a perfect cure in less than a month. The ointment of stearate of iron is made by mixing together a solution of sulphate of iron with a solution of Marseilles soap, drying the precipitate and then melting it, and finally adding some essence of lavender, taking care to stir until the mass has completely cooled. The adhesive plaster of stearate of iron is made by taking the stearate prepared as above described, melting it at a gentle heat, and spreading it upon linen as in ordinary plasters.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Bull. Gén. de Thérap.*, May 30, 1860.

12. *Remedies for Tape-worm in Abyssinia.* By Dr. Courbon.—The remedies for tape-worm stand at the head of the Abyssinian Materia Medica, and they are perhaps the only remedies that the Abyssinians are acquainted with, and that are really useful to them. Among these remedies, the kousso and mesenna must be ranked first, and afterwards the fresh bark of the pomegranate, the habi-tsalim, habi-tchogo, belbelta, and soaria. The *kousso* is furnished by a rosaceous plant of the tribe of the spiræas, and approximating to the genus agrimonia, and is called the *Brayera anthelmintica*. This is a beautiful diœcious tree, terminated above by a bunch of leaves, and with long, pendant bunches of flowers. The latter are the parts employed, and the dose is a handful. The Abyssinians reduce the kousso into a coarse powder, and swallow it mixed with water. An hour after taking it, there is an ordinary evacuation, and half an hour or an hour later there is a liquid motion, and at the end of three, or sometimes four or six hours, the tænia is expelled in the form of a whitish ball. The Abyssinians swallow the kousso fasting, and take no food until after the expulsion of the tænia, but afterwards they drink and eat abundantly, and use the most exciting kinds of food and drink. The *mesenna*, another remedy for tape-worm, was once supposed to be obtained from the *Juniperus procera*, but it is really the product of a leguminous plant, the *Albizzia anthelmintica*, a tree of some three or four feet high, with a thick and very rugous bark. The latter is the part used in medicine. The Abyssinians take it in various ways, but they always employ the powdered bark in the dose of about sixty grammes. They mix it with water, or they make it into bread with flour, or they incorporate it with butter or honey, so as to form a kind of bolus, which they swallow. The consequence is, that on the evening when the drug is taken there is a semi-solid motion, in which there are some fragments of the worm. It is only on the next day and the following days that the rest of the tænia is expelled in sero-mucous evacuations. The mesenna is said to be the best remedy for tape-worm, and completely expels this parasite from the body. The *habi-tsalim* is obtained from two kinds of jasmine—the *Jasminum Abyssinicum* and the *J. floribundum*—and the leaves are the parts employed, mixed with the young shoots of the *Olea chrysophylla*, a kind of olive. A handful of this mixture is pounded very carefully between two stones, with the addition of a little water, and thus a kind of liquid paste is obtained and swallowed by the natives. It is said to be very efficacious in expelling the tænia. The *habi-tchogo* is now proved to be the *Oxalis anthelmintica*, a handsome plant with a subterranean stem, terminated by an oval bulb of the size of a chestnut. The bulbs are the parts employed, in the dose of sixty grammes or more; the Abyssinians eat them by handfuls like small onions, or bruising them on a stone, they mix them with fluid, and drink the juice after it has been strained through them. The *habi-tchogo* is said to be almost as efficacious as the kousso. The *belbelta* belongs to the family of *amarantaceæ*, and is said to be obtained from the *Celosia adoensis*. According to Schimper, the leaves, flowers, and fruits are employed for the tænia; but MM. Ferrel and Galinier state that it is the powdered seeds which are used. The *soaria* is a small shrub belonging to the family of the *Myrtaceæ*, and is the *Mœsa picta*. The part employed is the fresh fruit, or the same part dried. The *roman* is the native name of the *Punica granatum*, or pomegranate. In Abyssinia, as in Europe, the bark of the root is the part employed, but it is rarely used. Among all these remedies for tape-worm, the

kousso, the mesenna, and the habi-tchogo are almost the only anthelmintics employed by the Abyssinians.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Bull. Gén. de Thérap.*, April 30 and May 15, 1861.

13. *Ergot of Wheat.*—Some of the practitioners at Claremont-Ferrand, in Auvergne, have of late substituted the ergot of wheat for that of rye with advantage. It is larger, rounder. and harder than the rye, and its odour is less disagreeable. Attracting moisture also far less than the ergot of rye, it retains its activity after being powdered for a longer period.—*Med. Times and Gaz.*, July 20, from *Gaz. Hebdom.*, No. 28.

14. *Ferri Carbonas Effervescens.*—Dr. THOMAS SKINNER, of Liverpool, believing that the proto-carbonate of iron is one of the best of the chalybeates, thought it desirable to obtain it nascent, and in a soluble form, and recommends for this purpose the following formula:—

℞.—Acidi tartarici ℥iij; Sodæ bicarbonatis ℥v; Ferri sulphatis ℥x; Pulveris sacchari ℥j; Acidi citrici ℥ij.

1. Mix the sulphate of iron with the sugar and part of the tartaric acid. 2. Mix the citric acid with the remainder of the tartaric acid and the bicarbonate of soda. 3. Add the mixtures, and thoroughly incorporate them by sifting. 4. The whole is now to be thrown into a metallic pan set in a water bath; in a few minutes it will separate, when it should be rapidly stirred until granules are formed. If preferred, it may then be flavoured with the oil of lemon; hitherto, however, the preparation has been without it.

Physical and other Properties.—When the above is carefully prepared, it has all the appearance of the popular and well known granular effervescent citrate of magnesia, with the addition of a slight yellowish green tint. Every drachm and a half contains ten grains of sulphate of iron, which, with a complement of bicarbonate of soda, is certain to produce, in a state of solution, four grains of nascent protocarbonate of iron. At the same time, there is developed a tartrate with a little citrate and sulphate of soda, which is, if anything, an advantage, as they act the part of a very gentle saline aperient, obviating the usual astringent effect of preparations of iron, as well as the too frequent constipation attending cases requiring chalybeates, particularly amongst females. The taste of it depends very much upon the amount of dilution. When taken in the dose and manner hereinafter recommended, the taste is that of a mild, sparkling, and refreshing chalybeate.

After the effervescence subsides, a perfectly clear, light-green solution remains, which, if allowed to stand for some time, becomes of a deeper green colour on the surface, gradually increasing from above downwards, and floating like a cloud upon the upper stratum of the liquid. This appearance was at first mistaken for oxidation, but the more correct explanation seems to be that it is the carbonate of iron which was retained in solution by excess of carbonic acid gas; that, as the excess escapes from the surface, the carbonate separates from the solution in the form of a fine cloud, and becomes ultimately precipitated in the form of an impalpable powder. So far as permanency is concerned, the preparation has stood the test of several months, and it now remains as good as the day when it was made.

Dose, Uses, Mode of Administration, etc.—The dose is a teaspoonful, more or less (about a drachm or a drachm and a half), twice or thrice a day, in half a tumbler or more of water, an hour after, between meals, or upon an empty stomach, as is found most suitable. Dilution, within reasonable bounds, increases the tolerance of the remedy and favours its therapeutic action. It may be drank during the action of effervescence, but it seems to me preferable immediately after it subsides. When a prolonged course of iron is required this preparation will never take the place of the protosulphate, the iodide, or the sesquichloride; but where a moderate course of a few days, or a week or two, is necessary, it will be well borne by the stomach. It is not only well borne, but it seems to produce a much more manifest chalybeate effect within a given time and in a smaller dose, than any other preparation of iron with which I have had experience.

In facial or other forms of neuralgia, arising from anæmia or other cause re-

lievable by iron, and particularly if the bowels are at all torpid, a few doses often act like a specific. It ought, however, to be continued after the pain disappears, so as entirely to remove the condition upon which the neuralgia depends. I may remark that the quantity of iron, and the aperient effect might be doubled if required; the present proportions, however, have been found by experience to be the best for ordinary purposes.—*British Med. Jl.*, June 15, 1861.

15. *Sinapisms made with Glycerine.*—M. Grimault recommends the following formulæ: Pure glycerine 3½ drachms, starch 5 drachms, and volatile oil of mustard 20 drops; and M. Chevallier publishes the following formula for what he terms *Plastic Sinapisms:* Volatile oil of mustard 20 drops, white pitch 15 drachms. Having melted the pitch, remove it from the fire, stir in the volatile oil, and spread on leather. By the addition of 10 per cent. of resinous oil the mass can be spread on linen like ordinary adhesive plaster. For use a piece can be cut off according to the size of the part to which the sinapism is intended to be applied. The effect is very speedy, and the proportions of the active principle may be varied.—*Med. Times and Gaz.*, July 20, from *Journal de Chimie Méd.*, June.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

16. *Treatment of the Fever in the Zambesi.*—Dr. McWilliam read to the Epidemiological Society (June 3, 1861) a note from Dr. D. Livingstone, dated Tette, Nov. 28, 1860, on the treatment of this fever.

"In 1850 I adopted the plan of giving quinine, mixed with a purgative, as the first step of the treatment, and was successful in the cases of two of my own children, and an English party whom we found at Lake Ngami, and of whom one had died before our arrival. I have lost the notes of my reason for adopting the practice, but I have been successful in every case I have met with since. The prescription employed is resin of jalap, calomel, of each eight grains, quinine c. rhubarb, of each four grains, mix well together, and when required make into pills, with spirits of cardamom. Dose from ten to twenty grains. The violent headache, pains in the back, etc., all are relieved in from four to six hours, and with the operation of the medicine there is an enormous discharge of black bile; the patient frequently calls it blood. If the operation is delayed, a dessert-spoonful of salts promotes the action. Quinine is then given till the ears ring, etc. We have tried to substitute other purgatives instead of resin of jalap and calomel; but experiments have only produced the conviction that aught else is mere trifling. No strength is lost in our march up the river of 600 miles on foot. An European would be stricken down in one day, and the next after the operation of the remedy would resume his march on foot. In some very severe attacks it was necessary for the patient to travel on a donkey, but after two or three days he would prefer to tramp it. We tried Warburg's tincture, which has a great reputation in India, but it causes profuse sweating, and does not cure the disease: the strength is also impaired. We had a good supply, by the kindness of one of our nobility, but I am compelled to say that it did not answer our expectations. The daily use of quinine is no preventive. We have seen many cases occur when the person was on the verge of cinchonism. I employed the foregoing remedy with success on the west coast, but made no fuss about it more than to make a general statement in the *Missionary Travels*. I was not quite sure that our fever was identical with that Dr. McWilliam encountered in the Niger, but the melancholy fate of a party of six missionaries at Linyanti, where six out of nine Europeans and four native attendants perished in the short space of three months, makes me fear that it is the same complaint as that which destroyed the officers of Commodore Owen in the Zambesi, those of Captain Tuckey, in the Congo, and the crews of the great Niger Expedition in that river. My companions, Dr. Kirk and Mr. C. Living-

stone, entertain the same idea of the value of our pills as I do. We wrote a paper for one of the medical journals, but the above sad case makes us anxious that the remedy should become more extensively known than it has been, and I do not know a better plan for effecting this than by communicating it to Dr. McWilliam. Those who may try the remedy will do well to remember that the above doses are for strong adults. I cured myself and native companions in this way during many long journeys between 1852 and 1856, and that the remedy has no bad effects on the system may be inferred from the fact that I have had no regular attack of fever since my return. I have had little illnesses, probably from exposure to malaria in its most intense forms, but nothing like what I formerly experienced; and I am of opinion that what we were all taught, not to give quinine till we had used the preliminary measures of relieving the bowels, was a mistake. [Query: Might the remedy not be applied to some of the fevers at home that arise in unhealthy localities?] Around every village in this country there is a large collection of human ordure during the dry season. This is swept into the rivers by the heavy rains, and you may guess the effect from hundreds of thousands of villages. The natives here do not drink water as the natives do on the banks of the Thames, but make holes in the sand to draw from. Possibly this has as much to do with the origin of fever as it has at home."

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17. *Process of Resuscitation after apparent Death.*—Dr. RICHARDSON, at the termination of his last Lettsomian Lecture, presented the following conclusions as the result of his investigations on the proper means of resuscitation:—

"In restoring animation, artificial respiration should always be first resorted to, in the hope that there may be still some remaining action of the heart. If artificial respiration fail, it need not be tried after fifteen minutes; it will merely cause emphysema. Whenever artificial respiration is used, it would be of great advantage to warm the air to a temperature of 130°. In recovery-houses for the recently dead, a heated room would be a most important addition. If electricity be tried, the positive pole should be passed through the jugular vein or one of the carotid arteries to the heart; while the negative pole, insulated to the tip, should be passed through the wall of the chest to the cardiac structure. The most probable means of resuscitation would be to inject fresh oxygenated blood by the arteries, towards the heart; but it remains still a desideratum to discover a fluid which, when warmed to 96°, will sufficiently represent blood."—*British Med. Journ.*, March 9, 1861.

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18. *Treatment of Delirium Tremens by large Doses of Tincture of Digitalis.*—Dr. T. B. PEACOCK, in an interesting paper (*Med. Times and Gaz.*, Aug. 3, 1861) on this subject, states that he has "gone over the records of St. Thomas's Hospital for the last two years, 1859 and 1860, and finds that 36 cases (32 males and 4 females) of delirium tremens were under treatment during that time. Of this number 9 (7 males and 2 females) proved fatal, giving the very high rate of mortality of 1 in 4, or 25 per cent. The treatment to which these cases were submitted varied considerably, but consisted generally (in 30 out of 35 cases) in the employment of opiates, solid opium, morphia, the tincture of opium, or the liquor opii sedativus, in combination with antimony, ipecacuanha, purgatives, ammonia, ether, and the stronger alcoholic stimulants, brandy and gin, etc. The opiates were, in some instances, several times repeated, but the doses were never very large. In some cases they were exhibited in the earlier, in others at the later periods of the disease, when other treatment of a more or less antiphlogistic character had been previously had recourse to.

"The opiate remedies I cannot but regard as often too indiscriminately employed in the treatment of the different cases which are ordinarily described as delirium tremens. If exhibited in small doses they are apt to give rise to excitement rather than to procure rest; and such is a frequent result in certain constitutions and particular states of system, even when the doses are large. When also we consider that in fatal cases of the disease, death is generally preceded by coma and convulsions, and, on post-mortem examination, the brain is usually found much congested, and there is not unfrequently serous effusion beneath the mem-

branes or in the ventricles, indicating that more or less active inflammation or congestion had existed, the exhibition of large doses of opiate medicines cannot but be regarded as fraught with danger. I find that of the 30 cases in which opiates were given, the mortality was not less than 1 in 3½, or 30 per cent.

"The use of the stronger alcoholic stimulants in the earlier stages of the disease, also appears rather calculated to increase than to lessen the excitement of the patient. So far as my experience serves me, the symptoms most commonly supervene after a violent or very prolonged debauch, and only rarely from the sudden cessation of the accustomed stimulus. In the former class of cases, then, the exhibition of more stimulus when the blood and tissues are already saturated with alcohol, can only aggravate the evil.

"This objection does not, indeed, apply to the use of opiates in moderate doses, and of stimulants, especially the milder alcoholic beverages, when, towards the end of an attack, the patient, who had previously been treated by mild antiphlogistic measures, still continues restless and is becoming exhausted. Even, however, under these circumstances, there are objections to the use of the alcoholic beverages, which should cause them to be avoided, if that can with safety be done, or any substitute can be employed.

"It is evident also that the disapproval expressed of the use of opiates in cases of active cerebral excitement, does not apply to the treatment of cases in which the condition of the general system and of the brain is the very opposite to that referred to."

He further says that large doses (half ounce) of the tincture of digitalis have been administered in St. Thomas's Hospital in six cases, "five of these being cases of idiopathic delirium in which the affection was the direct result of excessive drinking, while in the sixth the disease was traumatic, or supervened in a man of dissipated habits, shortly after he had sustained a fracture of the thigh.

"In four of these cases only one dose of half an ounce was given; in a fifth, the same quantity was repeated after an interval of twenty-two hours; and in the sixth, the dose first given was only two drachms, and half an ounce was exhibited after an interval of about twenty-four hours. In two of five cases of which I possess notes, the stronger alcoholic stimulants were given in the earlier periods of the attacks, and in all but one or two, ammonia and bark were prescribed after the digitalis had produced its effects. The ages of the patients treated were 23, 30, 33, 44, and 45. The last patient only was a female, and all recovered.

"In no instance were any alarming symptoms of depression induced; but the pulse usually soon fell remarkably in frequency, and acquired greater power and steadiness. The sickness subsided; the tremor became less, and the patient was much quieted. It did not, however, appear that the digitalis directly induced sleep, but it calmed the patient and sleep seemed to ensue naturally, after a longer or shorter interval. The patient did not, however, sleep so soon, so soundly, or for so long a period, as, from Mr. Jones' report, had been anticipated; and in two cases morphia was given before rest was procured; while, in a third, small doses of compound ipecacuan powder were exhibited at short intervals during the attack. This difference may be due to the less liberal employment of the remedy at St. Thomas's. Mr. Jones states that he generally repeats the dose in four hours, and occasionally in a smaller quantity, for a second time; whereas at St. Thomas's two full doses were only prescribed in one case, and in that instance the second was given after an entire day had elapsed.

"The kidneys did not appear to have their action increased by the digitalis; on the contrary, so far as could be ascertained, the flow of urine continued, after the remedy was exhibited, to be only small in quantity.

"I would remark that it would be altogether premature to venture a decisive opinion upon the use of the large doses of digitalis in this disease; but the facts seem to warrant the conclusions—

"1st. That the drug, when exhibited in full doses, does not by any means produce the amount of depression which our previous experience of its action, in small and frequently repeated doses, would have led us to expect; and

"2d. That the remedy, in conjunction with other means, may probably be very usefully employed in the treatment of certain cases of the disease, and especially

when it occurs in young and robust persons, whose strength has not been broken down by prolonged habits of intemperance; and particularly when it arises as the immediate result of excessive spirit drinking. Delirium tremens, like other acute diseases, occurs under varied and opposite conditions, and it would be alike opposed to sound theory and practice to extend the same treatment to all cases of the disease."

19. *Treatment of Acute Dysentery by Ipecacuanha.*—Dr. R. W. CUNNINGHAM, Assist.-Surgeon 4th Bengal Europeans, highly extols (*Edinburgh Med. Jl.*, July, 1861) the plan of treatment of acute dysentery advocated by Surgeon Docker, which he has lately employed in a number of cases. This treatment consists in the administration of ʒj—ʒjss of the ipecacuanha in one dose.

The plan of treatment is as follows:—

"On admission, a sinapism is applied to the epigastrium, and ɱxxx of tincture of opium are given, for the purpose of rendering the stomach more tolerant of the remedy. An hour afterwards, ʒj—ʒjss of ipecacuan is given in powder. Mr. Docker recommends it to be given half an hour after the tr. opii; but, by waiting for a whole hour, the medicine is retained longer, and produces a more powerful effect. As may be supposed, considerable nausea is the result; but vomiting does not generally come on until at least an hour afterwards, sometimes two hours. During this nausea, copious perspiration is produced over the whole body; the pulse becomes more full and soft, and less frequent; the countenance loses the expression indicative of suffering, so marked in acute dysentery; tenesmus and abdominal pains immediately cease, and the patient has no more stools for twelve, and sometimes for twenty-four hours. As soon as the nausea passes off, he expresses himself immeasurably relieved. The perspiration is kept up for a considerable time, and, of course, he feels languid and depressed for a little; but this has not, in any case that I have seen, proceeded so far as to call for any interference. When the patient next goes to stool, he does so without pain or tenesmus; the evacuations are fluid, and hold in suspension small masses of feces, but are entirely free from blood or mucus. Sometimes the first dose is not sufficient, the dysenteric stools returning after a short interval; in which case a repetition of the dose will be required. Chicken broth and arrowroot is the only diet allowed. In most of the cases that I have met with nothing further has been required, after the action of the ipecacuanha, than to keep the patient upon the above-mentioned diet for a few days, until the bowels recover their tone; or, if any tendency to debility manifested itself, a few doses of infusion of chiretta with nitro-muriatic acid have been sufficient to complete the cure."

Dr. C. says that none of the cases treated by him in this way suffered a relapse.

It should be remembered, that the cases treated by Dr. C. were in the first stage, and no destruction of tissue had yet taken place, nor had any complications occurred.

20. *Ipecacuanha in Diarrhœa and Dysentery.*—Dr. M'KIDD narrated to the Medico-Chirurgical Society of Edinburgh, June 5, a case of an individual who had suffered from diarrhœa for ten years. During this period all ordinary remedies had been employed, but without any good effect, and latterly the patient had ceased to take medicine, and had abandoned himself to despair. It then occurred to Dr. M'Kidd to make a trial of ipecacuanha. The medicine was administered in twenty-grain doses (reduced in a few days to ten grains), in the form of pill, every twelve hours, and the effect was most remarkable. By the end of the first week the diarrhœa was almost entirely checked, and the patient had a feeling of well-being such as he had not experienced for ten years. The cure had been permanent, upwards of three months having elapsed without any recurrence of the complaint.—*Edinburgh Med. Jl.*, July, 1861.

21. *Action of Phosphorus on Phthisis.* By R. P. COTTON, M. D.—The generally admitted tonic and stimulating action of phosphorus, as well as its existence in so many of the important structures of the body, made me anxious to

try its effects upon phthisis. I accordingly administered it to twenty-five unselected in-patients of the Consumption Hospital, those only being excluded in whom the disease was too far advanced to expect that any kind of treatment would avail much, or in whom there existed some gastric or other complication, which would seem to render the phosphorus inadmissible.

Of the twenty-five patients, ten were in the first, four in the second, and eleven in the third stage of consumption. Eleven were males, and fourteen females. Their ages varied from nineteen to fifty-five years. Perhaps I should here mention the mode in which the phosphorus was administered. Ten grains of phosphorus and an ounce of oil of sweet almonds, were put into a phial, and exposed to the heat of boiling water for half an hour; the solution, when cold, being carefully strained through blotting paper. By this process, which is that recommended in the Prussian Pharmacopœia, four grains of phosphorus remain in permanent solution. Of this, from five to eight or ten minims were given twice or three times a-day, suspended in a little mucilage, and disguised by cinnamon water. Five minims would contain one-twenty-fourth of a grain of phosphorus. The dose, according to its effects, was continued for a longer or shorter period, the longest being eight weeks, the shortest, one week.

Of the twenty-five patients, four improved *greatly;* five improved *slightly;* and sixteen either made no progress at all, or became worse. Nine gained in weight; twelve lost weight; and four remained unchanged in this respect. In one case the gain amounted to eight pounds in one month; in another to fourteen pounds in two months; but in each of the other cases where there was an increase, the gain was trifling, not exceeding one pound.

In eleven instances the appetite sensibly diminished, and in two or three of these cases the phosphorus evidently occasioned gastric disturbance, accompanied by diarrhœa; but in the other fourteen patients, the appetite remained good, and in some of them actually improved under the treatment.

Of the four *greatly* improved cases, one (in the first stage of the disease) quite regained his health; and the other three (two of whom were in the first, and one in the third stage) left the Hospital materially improved in every respect, their coughs having diminished, and their general health being greatly restored. Of the five *slightly* improved, some gained a little in weight, but three at least of them seemed to profit more when the phosphorus was exchanged for steel and quinine. Of the sixteen cases apparently uninfluenced by the phosphorus, several improved under the subsequent use of other tonics.

The observations of my late esteemed and lamented colleague, Dr. Theophilus Thompson, upon the use of phosphorus in phthisis[1] are far from encouraging. He believed that, in some instances, it might act favourably as a stimulant, but that its effects even in such cases were transient. I am not aware of any other published record of the influence of this substance in any of the varieties of tuberculosis.

All things being considered—the fact that improved diet, diminution of care, and quietude, are of themselves sufficient to effect a very great improvement among the patients at the Consumption Hospital, especially on their first arrival, I cannot help coming to the following conclusions, viz:—

1. Phosphorus exerts no *specific* action upon consumption.

2. In some cases it seems to act as a tonic and stimulant; but its influence, even in this respect, is inferior to many other remedies of a similar kind.

3. Although in many cases it seems to agree very well with those who take it, yet it sometimes occasions loss of appetite, nausea, and abdominal derangement.—*Med. Times and Gaz.*, July 6, 1861.

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22. *Opium in Large Doses, and Inhalation of Chloroform in Idiopathic Tetanus.*—According to the results of experience, opium is still the most efficacious remedy in the treatment of idiopathic tetanus, and its use may be continued with safety for a long period. Two cases which occurred in the practice of M. Grisolle appear to leave no doubt upon this point; and one of the cases presents this peculiarity, that it shows how much superior opium is to inhala-

[1] Clinical Lectures on Consumption, p. 97.

tions of chloroform, to which latter great efficacy was at first attributed. The fact is, that although the inhalations of chloroform suspend for a short period the symptoms of tetanus, they do not remove them altogether, and with the elimination of the chloroform the disease resumes its primitive character and pursues its course. The two cases recorded were those of a man and a woman, both of whom, without any appreciable cause, were attacked with tetanus. In the first, which was that of the man, inhalations of chloroform were repeatedly employed, with temporary relief; but as soon as the effects of the anæsthetic passed away, the tetanic convulsions reappeared. At last, opium was given in the dose of five centigrammes every hour (a centigramme is .1543 of a grain); and after some days of this treatment, during which a large quantity of opium was taken by the patient, the tetanic symptoms disappeared, and the patient completely recovered. In the other case, that of the woman, the opiate treatment was pursued from the first, the bowels, however, being relieved by castor oil and purgative injections, and after persevering in this treatment for some days this patient was also cured.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Bull. Gén. de Thérap.*, Jan. 15, 1861.

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23. *Perchloride of Iron in some Cutaneous Affections.*—M. DEVERGIE, of the Hôpital St.-Louis, has observed some very satisfactory results from the use of perchloride of iron in the treatment of certain cutaneous diseases, and other practitioners have confirmed the merits of this kind of treatment. The two following cases are recorded by M. BOURON des CLAYES, of Créteil. The first was that of a man, sixty-five years old, who was suffering from eczema, accompanied with insupportable itching, extending from the scrotum to the margin of the anus. Poultices of potato-starch, ointments made with tar, and oil of cade were employed without success, and at length M. Bouron des Clayes lightly touched all the affected surfaces with a camel's hair pencil dipped in liquid perchloride of iron, over which he laid a thick coating of collodion. On the next day, the skin was a little brown, and completely dry over the whole extent of the eczema, and a fresh application was made of the perchloride and of collodion. The day afterwards, nothing remained except the colour of the skin to indicate that the patient had been affected with any eruption. The second case was that of a strong man, forty-seven years old, who suffering from itch, but who had also had for a long time *lichen agrius* of the forearm. M. Bouron des Clayes applied an ointment composed of perchloride of iron, bicarbonate of soda, and hog's lard, and at the end of a few days the itch had completely disappeared, the lichen was altogether improved, and he was rapidly cured.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Gaz. des Hôpitaux*, January, 1861.

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24. *Pathology of Cholera, with an Exposition of the Causes that necessarily render all Medicine inoperative in that Disease.*—In a paper read by Dr. TOULMIN, of Brighton, before the Harveian Society, the author commenced by remarking that in medicine as in other sciences, dogmas were sometimes received as truths without having been proved to be true, and that this sadly impeded the advancement of knowledge. That in his present inquiry into the phenomena of cholera, he found one of these time-honoured beliefs obstruct his progress, and which it became necessary to refute before he could expect his theory of the disease would be honoured with the approbation of the Society. The dogma he referred to was, that in all cases death must necessarily ensue on the cessation of respiration or the circulation of the blood. To this he objected to this extent, viz., that where black blood was not being carried to the lungs or brain, death did not necessarily ensue on the cessation of those functions, but that a passive state of existence might continue for many hours, or even days, after these functions had completely ceased. As a proof of this, he instanced the act of fainting (which sometimes was of long continuance), drowning, the hybernation of animals, and more particularly the state of trance, of which he read the curious and interesting case of the Hon. Colonel Townshend, as given by Dr. Cheyne. He admitted that life consisted in a never-ending series of phenomena, not one of which could be arrested singly without the consequent death of the individual; but he proposed a case where, in full health, all the functions should be simul-

taneously suspended, where, although respiration had ceased, no carbonized blood was suffused on the lungs or brain, because the veins had ceased to carry any—where, although the secretion of urine was stopped, still no uremic poisoning ensued, because there was no metamorphosis of tissue going on to produce it; and then asked, what was there in such a case to produce immediate death? Life might be suspended, but not lost; and he contended that in a similar state to this persons continued to exist for a certain time in the collapse of cholera, and were thus frequently buried alive.

He then desribed cholera as a certain poison, *sui generis*, which was received into the blood, and which attached itself particularly to the serous portion of that fluid; but observed, that however virulent the nature of the poison might be, still all the subsequent phenomena, terminating even with the death of the individual, arose from a series of consecutive causes of the most simple and natural kind, originating in nature's efforts to expel this poison from the system through the medium of the alimentary canal—the rice-water evacuations being, confessedly, nothing more than serum derived from the blood. From these facts he drew the following important deduction—viz., that any attempt to arrest the diarrhœa of cholera was not only futile, but mischievous.

He then observed, that all the subsequent symptoms of the disease might be traced to this one conservative act of nature: as, first, the rapid cooling of the body, which arose from the absolute cessation of all combustion of matter; that this, again, was caused by all metamorphosis of tissue being arrested, which in like manner depended on the sudden congelation of the blood, as the necessary consequence of the sudden and total loss of all its serum, the very celerity of which cut off all further supply. The absolute cessation of the circulation of the blood, he remarked, was due to this one cause, conjoined to another, either of which was equal to the occasion—viz., the shock given to the heart's action at the moment the poison was first received into the system.

The author then remarked, that in such a state as this the patient ceased to be governed by the laws of animal life, and that the attempt to keep him warm, after his own power of generating heat was lost, by wrapping him in blankets, was as unphilosophical as it would be with a marble statue.

As a demonstrative proof of the absolute stagnation of the blood, and that all change of matter depending thereon had ceased, he instanced the state of respiration, the air inspired returning unaltered; the rapid loss of heat, as well as the power of generating it; the arrest of the secretion of urine, as well as of bile, and, indeed, of all functional activity. As a proof of the latter, he noticed that mercury never produced ptyalism, nor opium sleep; that calomel, when given, was found adhering to the lining membrane of the stomach unabsorbed; and he believed that in the state of collapse any poison might be given (not acting mechanically) with impunity, until reaction was again established, when its specific effect would for the first time become apparent, to the serious detriment of the patient.

The extreme thirst always attending on cholera he explained as the inevitable consequence of the great loss of fluid in the system; and cramp, as urea deposited amongst the fibrillæ of the muscles before combustion had entirely ceased, remaining unabsorbed.

In considering the treatment of the disease, he divided the period of its existence into two stages: the first (and where medicinal treatment was all important) extending to the commencement of collapse; the second as being the state of collapse itself, to which alone all his observations had reference, and in which the exhibition of medicine was as useless as it would be in giving it to an automaton. The author then asked what, pathologically speaking, the system most loudly called for? the answer to which, not only from science, but also from the patient himself, was—*water*, to dilute the crassamentum, and thus re-establish the circulation of the blood, which when once effected, life was saved. But how, he inquired, was this to be effected when all power of absorption was lost? By the endosmose of tissue, and by this alone. By this function, he observed—which plays so important a part in the animal economy, and, as is well known, is equally active in dead as in living membrane—the fluid swallowed is, in the most direct and simple manner, at once imbibed in its course through the ali-

mentary canal by all the arts requiring it—by the coats of the bloodvessels, the areolar tissue, the abdominal cavities, and the viscera in general; and thus the whole system is supplied with its due degree of moisture, and this without any assistant action on its part. The author pointed out four ways by which water might be imbibed: by the mouth, the skin, the rectum, and by its injection into the veins themselves; but only drew the attention of the Society to the first two, which in practice, he observed, he had found all-sufficient. By the mouth, the ardent thirst of the patient imperatively called for water icy cold, and in fact his whole time should be employed in sucking ice and drinking water; whilst by wrapping him in the hydropathic wet sheet the whole surface of the skin imbibed water in the most subtle and effective manner. By these means, he observed, he had never seen a case in which reaction had not been established. At the same time he commented on the potency of the remedy, and urged the importance of seizing the golden moment when reaction was established to remove the patient from the wet sheet, and to place him in a room hotter than his blood, or in a warm bed, and then supplying him with some mild stimulant, in the selection of which he would rather be guided by the wish of the patient than his own fancy.

Throughout the paper the object of the author appeared to be, to insist on the occurrence of an absolute cessation of all capillary action, and consequently of the larger circulation of the blood, which he said gave the key to all the subsequent symptoms, and at the same time removed cholera out of the category of disease in general, necessitating a mode of treatment in which vital action was not required.—*Lancet*, Aug. 7, 1861.

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25. *Period of Incubation in Smallpox.*—Professor Dr. VON BARENSPRUNG gives the abstract of several cases, showing that in smallpox the interval of time between the infection and the eruption is always the same even under the most varied circumstances. It is well known that in cases of *inoculation* the local affection began about the fifth day; from the eighth to the ninth the eruptive fever set in; and from the tenth to the eleventh the general eruption. But the action of "*contagion*" has been considered as less uniform, and the period of incubation has been variously estimated by writers. For example, Naumann states it to be three days; Wilson, at from four to twenty days; Canstatt, at from eight to fourteen; Heim, at nine days; Hufeland at fourteen days; whilst according to Huxham, the eruption may be delayed for a month after the infection. Moreover, whilst fourteen days is considered by most as the medium period of incubation, it is considered that the character of the epidemic, as also the greater or less susceptibility of the patient, may modify it. The cases adduced by the author are certainly remarkable, and have all the character and value of direct experiment; they are as follows: On the 20th of January, 1851, a female who had never been vaccinated fell ill of confluent smallpox eight days after coming to stay in Halle, in which place, at the time, no cases of this disease existed. She died on the sixth day. On the 27th the body was examined post-mortem by Dr. Meckel, in the presence of several students and medical men; and as a result of the examination, no less than seven persons became ill of the smallpox. Of these, four had been present at the inspection, and three others were persons in close intimacy with those who had been present. In every one of these cases the disease broke out at the same time—namely, between from twelve to thirteen days afterwards.

A brief and condensed summary of these cases will be read with interest.

CASE I. A student began to be affected on the evening of the 8th of February; on the day following the fever greatly increased; on the morning of the 11th a roseola on the hands and feet appeared; on the 12th numerous red knots were seen over the whole body, which soon became actual pustules. The patient was well at the end of the month.

CASE II. A student felt unwell on the 8th of February, and on the 9th was in a high state of fever. The eruption appeared on the 13th. Termination favourable.

CASE III. A student was taken ill on the 9th of the month. The eruptive fever was active, but the pocks only very scanty. Termination favourable.

Case IV. A physician was affected with fever during the night, between the 8th and 9th of the month. Eruption on the 13th. Termination favourable.

Case V. A student who lived with one of those who had been present at the post-mortem examination, but who was not there himself, being previously in sound health, became ill on the 8th, and on the tenth was in a state of high fever, with headache. Two days later the pocks broke out. Termination favourable. He had been vaccinated.

Case VI. A physician who was present at the inspection had carried home a piece of the skin affected with the disease for examination. He remained unaffected himself, but his wife, who was pregnant, and who had been vaccinated in her youth *and only a year previously, with good results*, began on the 8th of the month: day following high fever. On the 10th delirium; on the 11th a scarlatina-like rash on the skin, delirium at evening; and on the 12th she was prematurely confined. On the 13th, remission of fever and lessening of the skin affection. The child lived fourteen days, and was unaffected by the disease.

Case VII. Was the child of the attendant at the post-mortem examination, who had sewn up and washed the body. It had not been vaccinated, and although its father and mother remained unaffected, fell ill on the 8th of February; on the 11th the eruption began, and on the 12th the whole body was covered. It died on the 14th with pulmonary symptoms.

Such are the various cases related by the author, which are so uniform in their nature, and occurred under circumstances so favourable to precise and scientific observation as to afford material for most trustworthy and valuable deduction. As he remarks, it is not a little remarkable that seven people so differently circumstanced and predisposed, some being vaccinated, others not so; some being adults, others children; some male, and others female, should, in spite of these differences, be all affected at the same period after exposure to contagion (and this notwithstanding that the intensity of the contagious influence was very unequal), for in every case the outbreak was between the thirteenth and fourteenth day after. The development of the skin-inflammation in the numerous cases was less uniform.

The author closes by relating one or two other cases which completely support the above observations, and which occurred under his own immediate notice in 1858.—*Brit. and For. Med.-Chir. Rev.*, July, 1861, from *Annalen des Charités Krankenhauses.* Band xix.

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26. *Cases illustrating the Causes and Effects of Obstructions in the Arteries, both of the Brain and of other Organs.*—Mr. S. W. Sibley made a communication to the Royal Medical and Chirurgical Society (June 25, 1861), the object of which was to review a series of cases which had occurred in the Middlesex Hospital, with the view more especially of testing the theory which ascribes these obstructions to the washing away of vegetations, etc., from the valves of the heart. The cases were divided into four groups. In the first group (eleven cases) were placed those instances in which softening of the brain was found associated with a plug in one of the cerebral arteries. In the second group were placed three cases in which there was softening of the brain associated with vegetations on the valves of the heart; but in these instances the state of the cerebral arteries was not ascertained. The third group was formed by two instances of cicatrix of the brain, which the author believed to have been produced by the plugging of a cerebral vessel. In the fourth group were placed ten instances in which there were fibrinous deposits in the internal organs, but in which the brain was not affected with softening. The cases were then analyzed. It was found that in twelve out of the fourteen examples of softening of the brain there were fibrinous deposits in the internal organs; the remaining two cases were not complete. It also appeared that, in twelve out of the fourteen cases of softening of the brain there were warty growths on the valves of the heart. In one of the remaining cases there was extensive atheromatous and calcareous disease of the aorta; in the other the heart and vessels were healthy, but there was hepatization of the lung. Other points relating to the fibrinous deposits in the organs were also analyzed; and this was followed by a comparison of the symptoms in the cases of softening of the brain. The author pro-

ceeded to describe the phenomena which followed the sudden obstruction of an artery. The complete arrest of circulation, the attempt at its restoration, and the reason why in the cases of the brain, spleen, and kidney this attempt is not successful. The partially restored circulation is characterized by a zone of enlarged vessels around, and by a low form of inflammation in the part affected. In consequence of this the nutrition of the part is damaged, and fatty granules accumulate among the cell-structures, thus causing the bright yellow colour which is seen in the so-called fibrinous deposits. As the circulation becomes more complete, the more plastic products of inflammation are formed, the yellow colour fades, the bright zone of enlarged vessels slowly disappears, and at length a cicatrix is formed. The paper concluded with a review of the arguments for and against the theory which supposes these fibrinous obstructions to have been washed away from the cavities of the left side of the heart. The author believes that obstructions may be formed in the arteries, or that they may be washed away from the heart; and after describing the mode in which obstructions formed in these two modes are to be distinguished from each other, he proceeded to give the reasons for affirming that in all the cases mentioned in this paper the plugs had come from the heart, from the arteries, or from an inflamed lung. The chief arguments made use of for arriving at this conclusion were: the peculiar appearance and structure of the plugs; their analogous structure to warty growths on the valves of the heart; the situation at which plugs are usually met with; the condition of the artery at the obstructed part; the occurrence of several plugs in neighbouring or distant vessels; the very frequent, or indeed almost constant, association with fibrinous deposits in the spleen and kidney, and, lastly, the arguments which are derived from a consideration of the symptoms of this form of brain disease.—*Med. Times and Gaz.*, July 6, 1861.

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27. *Case of Traumatic Emphysema of the Liver.*—A case, probably unique, was recently related to the Paris Society of Surgery by M. CHABERT. A drunken artilleryman threw himself from a window and died three days afterwards from the numerous injuries received, a marked jaundiced condition of the skin previously manifesting itself. At the autopsy a multitude of small, irregularly-shaped elevations of a brownish-yellow colour covered the external surface of the liver. When punctured a noise like that from the bursting of a small bladder filled with air was heard. There were three lacerations of the organ two or three millimetres only in depth. Incisions into various parts of the liver gave issue to no fluid, their surfaces being dry and of a brownish-yellow colour, and presenting numerous vacuoli like a sponge. On pressing portions of the organ between the fingers crepitation was distinctly heard. The diminution of specific gravity was remarkable, portions of any part of the liver floating in water, and when compressed under this yielding numerous bullæ, which burst on the surface, the liver then sinking to the bottom. The surface of the gall-bladder (distended with deep-coloured bile) was covered by a great number of vesicles resembling those of the surface of the liver. No cadaveric change accounted for these appearances, as the autopsy was performed soon after death. The diaphragm and the intestines being completely uninjured, it cannot be admitted that the air contained in the liver could have proceeded from either the lungs or the intestines. M. Cruveilhier's failure in his experiments to inject the biliary passages from the duodenum forbids the supposition that the air gained access to the hepatic parenchyma by a reflex passage through the choledochus. We are thus driven to the supposition that the gas must have been formed within the liver itself, probably within the cellular tissue connecting the Glisson's capsule with the glandular tissue. In the *Union Médicale*, September 8, 1855, cases are related in which emphysema of the liver was connected with internal disease; but there is no other example of its dependence upon a traumatic lesion.—*Med. Times and Gaz.*, August 17, 1861, from *Union Méd.*, No. 84.

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28. *Ozæna.*—Dr. DEMME, of Berne, states that he has seen a number of cases of ozæna, successfully treated by touching the nasal mucous membrane with saponine (a preparation of tar), and by injecting this same article into the nose.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

29. *Statistics of Amputation.*—Dr. T. P. Teale has published (*Med. Times and Gaz.*, July 6, 1861) the statistics of amputations at the Leeds General Infirmary during the eight years preceding 1861. The operations were all performed by Dr. T., Mr. S. Smith, and Mr. S. Hey. Amputations at the joints are not included.

All the Amputations performed at the Leeds General Infirmary from Jan. 1, 1853, to Jan. 1, 1861.

	PATHOLOGICAL.			EXPEDIENCY.			PRIMARY.			SECONDARY.			TOTAL.			
	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Total.	Cured.	Died.	Per cent. and average.
Thigh . . .	27	6	18.18, or 1 in 5.5	2	1	33.3, or 1 in 3	2	5	71.4, or 1 in 1.4	..	6	..	49	31	18	36.7, or 1 in 2.7
Leg	50	7	12.2, or 1 in 8.1	1	1	50, or 1 in 2	10	3	23, or 1 in 4.3	..	2	..	74	61	13	17.5, or 1 in 5.7
Arm	4	2	33.3, or 1 in 3	..	..	..	17	9	34.6, or 1 in 2.8	1	1	50, or 1 in 2	34	22	12	35.3, or 1 in 2.8
Forearm . .	9	..	..	..	..	..	16	1	5.9, or 1 in 17	5	1	16.6, or 1 in 6	32	30	2	6.2, or 1 in 16
	90	15	14.3, or 1 in 7	3	2	40, or 1 in 2.5	45	18	28.5, or 1 in 3.5	6	10	62.5, or 1 in 1.6	189	144	45	23.8, or 1 in 4.2

The unusually large mortality of amputations of the arm, Mr. T. says, is explained by the fact that of 34 amputations of the arm, 28 were traumatic, having generally resulted from severe accidents by machinery, often attended with serious injuries of other parts of the body.

The *causes of death* in the 45 fatal cases were as follows: Pyæmia, 24; secondary hemorrhage, 4; exhaustion, 4; shock, 4; other causes, 5; causes of death not recorded, 4.

Comparison of Rectangular with other Amputations.—Mr. T. gives the following tables illustrative of this:—

Amputations by other Methods than the Rectangular.

	PATHOLOGICAL.			EXPEDIENCY.			PRIMARY.			SECONDARY.			TOTAL.			
	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Total.	Cured.	Died.	Per cent. and average.
Thigh . . .	6	1	14.28, or 1 in 7	..	..	..	1	5	83.3, or 1 in 8.2	..	1	..	14	7	7	50, or 1 in 2
Leg	20	3	13, or 1 in 7.6	..	1	..	1	3	27.2, or 1 in 3.6	..	2	..	37	28	9	24.3, or 1 in 4.1
Arm	1	1	50, or 1 in 2	..	..	..	11	7	38.8, or 1 in 2.6	..	..	..	20	12	8	40, or 1 in 2.5
Forearm . .	4	..	..	..	..	..	8	1	11, or 1 in 9	1	1	50, or 1 in 2	15	13	2	13.3, or 1 in 7.5
	31	5	13.9, or 1 in 7.21	..	1	..	28	16	36 3, or 1 in 2.7	1	4	80, or 1 in 1.25	86	60	26	30.2, or 1 in 3.3

In these 86 amputations by other methods than the rectangular there were 26 deaths, a mortality of 30.2 per cent. In 103 rectangular operations there were 19 deaths, a mortality of 18.4 per cent.

If we subdivide these two groups the following results are obtained:—

Of the 86 amputations by other methods than the rectangular, 37 were pathological with 6 deaths. a mortality of 16.2 per cent. Of 103 rectangular amputations 73 were pathological, of which 11 died, a mortality of 15 per cent.

Of the 86 amputations by other methods than the rectangular 49 were traumatic with 20 deaths, a mortality of 40.8 per cent. Of 30 rectangular traumatic amputations 8 died, or 26.6 per cent.

Remarks.—On reviewing the foregoing statements, additional confirmation is obtained of a fact already well established, that traumatic amputations are much more fatal than pathological; the rate of mortality at Leeds in the traumatic being 35.4 per cent., in the pathological 15.4 per cent.

Rectangular Amputations.

	PATHOLOGICAL.			EXPEDIENCY.			PRIMARY.			SECONDARY.			TOTAL.			
	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Cured.	Died.	Per cent.	Total.	Cured.	Died.	Per cent. and average.
THIGH . . .	21	5	19.2, or 1 in 5.2	2	1	33 3, or 1 in 3	1	..	..	..	5	..	35	24	11	31.4, or 1 in 3.18
LEG	30	4	11.7, or 1 in 8.5	1	..	..	2	..	..	..	..	..	37	33	4	10.8, or 1 in 9.2
ARM	3	1	25, or 1 in 4	..	..	..	6	2	25, or 1 in 4	1	1	50, or 1 in 2	14	10	4	28.5, or 1 in 3.5
FOREARM . .	5	..	..	..	..	..	8	..	..	4	..	..	17	17	..	..
	59	10	14.5, or 1 in 6.9	3	1	25, or 1 in 4	17	2	10.5, or 1 in 9.5	5	6	54.5, or 1 in 1.8	103	84	19	18.4, or 1 in 5.4

In the traumatic cases the secondary proved much more fatal than the primary; the rate of mortality of the secondary being 62.5 per cent., of the primary 28.5 per cent.

On comparing the mortality of rectangular with that of other amputations, the advantage is here shown to be obviously on the side of the rectangular, although the general results in this respect are scarcely equal to those obtained in the earlier of these operations.

The more favourable results which have hitherto attended the rectangular method, I am inclined to attribute chiefly to two causes; first, to the more complete covering of the end of the bone secured by this method; and secondly, to the avoidance of irritation and distress in the subsequent treatment; the repeated liftings of the stump, the strappings, and the circular bandagings, usually required in other amputations to prevent the retraction of the soft structures, being unnecessary in the rectangular.

In hospitals, when erysipelas and pyæmia are prevailing, stumps frequently "open out," suppurate, or become phagedenic. But when rectangular stumps are thus affected, and life is preserved, the resulting stump is, with very rare exceptions, a good one. In several cases, apparently most unpromising, although the stump has lain long without showing a disposition towards repair, yet when recovery was at length accomplished, there was a soft movable mass of tissues over the end of the bone, almost as satisfactory as in the cases which had done well from the beginning.

After ordinary amputations it is admitted by surgeons of great experience, that exfoliation and necrosis are *frequent*. After rectangular amputations they are *rare*. After 103 of these operations, in two cases only, and to a very small extent, have exfoliation or necrosis come under the knowledge of my colleagues or myself.

30. *Complete Cure of a Congenital Total Cleft of the Hard and Soft Palate by a New Operative Proceeding.*—Professor LANGENBECK exhibited at a recent meeting of the Medical Society of Berlin, a case of this. The patient, a boy aged 14, had been born with a cleft of the left lip and total cleft of the hard and soft palate together with the left alveolar process. The operation of hare-lip had been done soon after birth, but had failed; and a second operation made about a year afterwards, had been successful. On February 6 M. Langenbeck united first the cleft of the soft palate by means of a needle-instrument especially invented by him for this purpose. On May 11 he closed the hard palate in the following manner: Close to the edges of the cleft, the mucous membrane and the periosteum of the hard palate were cut through to the bone by means of a strong knife, and the edges of the cleft freshed up. By means of a raspatorium placed into these incisions, the mucous membrane and the periosteum were then drawn off and separated by blunt levers from the two halves of the hard palate. Thus two flaps were formed which were only connected anteriorly with the gums at the alveolar process, and proceeded backwards into the soft palate which had been separated from the posterior margin of the os palatinum. The edges of these flaps were then united by five silken sutures in the median line, and thus the whole cleft, up to the front teeth completely covered up. This separation of the mucous membrane of the palate and of the periosteum is troublesome, but can be done without in any way injuring other parts. The hemorrhage from the vessels of the periosteum necessitated repeated interruptions of the operation, which was, however, concluded in half an hour. After the sutures had been removed, which was done in the course of the second week, the cure by first intention was quite complete in the whole extent of the wound. The healed palate is only distinguished from a normal one by a fine cicatrix visible along its whole extent and a slight indentation at the top of the uvula. In another case of total cleft, in a woman aged 24, who came under M. Langenbeck's care a fortnight afterwards, he performed staphyloraphy and uranoplastics in the same sitting, and with the best result.—*Med. Times and Gaz.*, July 20, 1861.

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31. *Partial Amputation of the Foot.*—Professor LINHART, at a late meeting of the Physico-Medical Society of Würzburg, read a very interesting paper on the above subject. He introduced four male patients, on whom partial amputations of the foot had been performed, with the best success. The first case was one of tarso-metatarsal disarticulation according to Lisfranc; the second one of disarticulation in the tarsus according to Chopart; the two others, amputations with preservation of the posterior process of the calcaneus (or osteoplastic elongation of the leg according to Pirogoff). All these four patients walked firmly on the operated leg and without feeling the least pain. In the first there was scarcely any deviation from the normal gait to be seen; the third and fourth walked as patients do, if the tibio-tarsal joint is anchylosed; and in the second, on close observation, a sort of double tread was remarked, as he first treads on the tuber of the calcaneus, and then on the anterior part of the soles. This symptom was caused by the tuberositas of the calcaneus being lower down than the head of the astragalus and the anterior process of the calcaneus. Walking was, however, not in the least troublesome.

Professor Linhart said, that the foot, in standing, touches the ground with the tuberositas of the calcaneus and the capitula of the metatarsal bones only; the ligamentum calcaneo-cuboideum and calcaneo-naviculare plantare, which form the vault of the sole, bearing the weight of the body, like the springs do a carriage. The capitulum of the metatarsal bone of the great toe is the greatest and most important part of the anterior support of the foot. This explains why persons, in whom the metatarsal bone of the great toe has been removed, have a more uncertain tread than such in whom two or three other metatarsal bones have been taken away. The shape of the foot is similar to a spire, so that the back of it passes into the external, and the sole into the internal, surface of the heel. From these facts the professor established the rule that in disarticulations in the neighbourhood of the tarsus, the flaps ought to have an oblique direction, so that an external upper dorsal, and an internal lower plantar flap is to be formed, if we

wish to avoid the inconvenience of drawing the soft parts of the sole into the flap of the back, and *vice versâ*.

The professor then spoke about the so-called longitudinal amputation. He showed, on a foot just taken from a dead body, how we may, from the interval between the third and fourth toe, divide the foot in two longitudinal halves, of which the external contains the metatarsal bones of the fourth and fifth toe, the os cuboideum and the calcaneus, while the internal half consists of the three inner metatarsal bones, the os naviculare and the astragalus. Such a total longitudinal amputation has never yet been performed on a patient; professor Linhart, however, considers it practicable and likely to answer very well; only there will be very few cases in which it could be indicated. The longitudinal amputations hitherto performed have been confined to the removal of some metatarsal bones with the os cuboideum and the sphenoid bones.

In transverse amputations, the chief rule is to preserve as much as possible of the limb, as experience shows that all transverse amputations yield favourable results. It is much to be regretted that Chopart's operation has been so seldom performed in recent times; there is no tangible reason for this to be found, but it seems to have gone out of fashion. The reasons adduced for neglecting it may be easily refuted; and if in former times frequent relapses of caries, impossibility of walking, and other unpleasant symptoms were observed, the cause was that the operation, after having come into fashion, was performed too frequently and without the right indications. The permanent drawing up of the heel, which has been often mentioned as a consequence of Chopart's operation, is certainly very rare. Professor Linhart has never seen it; in one case only he noticed a slight painful spasmodic drawing up of the heel by the tendon of Achilles.

With regard to the tibio-tarsal disarticulation, the professor said that the operation, according to Jaeger and Syme, with enucleation of the calcaneus, should only be performed if this bone were also diseased; in other cases Pirogoff's method should be preferred. The objections made to this latter operation are entirely unfounded, especially that the sawed surfaces did not unite. In a patient introduced by M. Linhart, suppuration had set in on these surfaces, and, nevertheless, osseous union took place in the end. The greater length of the limb, after Pirogoff's operation, is very important. The abscesses which may form are of no consequence if at once recognized and opened. The diagnosis of them is, however, not always easy, and frequent observations and examination of the leg and foot is required for it. This circumstance is the chief objection to be raised against the application of the gypsum-bandage in such cases; and it is better to place the diseased foot on a cushion which is hollowed like a groove.—*Med. Times and Gaz.*, July 20, 1861.

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32. *Statistics of Operations for the extraction of False Cartilages in or about the Knee-joint.* M. HIPPOLYTE LARREY has communicated the following interesting statistics on this subject, reaching from the days of Ambrose Paré to our own time.

Out of a total of 168 cases of extraction, 129 were by direct incision, and 38 by the indirect or subcutaneous method. The results stand thus:—

No. of Cases.	Cured.	Failures.	Deaths.
129	98	5	28
38	19	15	5

The extraction of false cartilages of the knee-joint is, according to M. Larrey, a grave operation when practised by direct incision, and difficult when by the subcutaneous method. The dangers resulting from the presence of the foreign body are much less to be dreaded than those to be apprehended from the operation; and though, no doubt, as seen by the above figures, a large proportion of cures has been registered, many failures have been passed over in silence, and not boasted of. The operation should never be performed unless the following conditions be realized:—

1. Complete mobility of the false cartilage.

2. Presence of pain, effusion into the joint, lameness, and other ill effects resulting from its presence in the articular cavity.

3. Failure of palliative measures.
4. Express desire of the patient to undergo an operation, after all its dangers and difficulties have been fully explained.—*Lancet*, June 15, 1861, p. 599.

33. *Radical Cure of Inguinal Hernia.*—Holmes Coote, Esq., Ass. Surg. St. Bartholomew's, brings forward (*Lancet*, June 1, 1861) some striking facts, to show that Wützer's operation for inguinal hernia cannot be depended on for a permanent cure.

"Those," he remarks, "who are in the habit of frequenting the wards of St. Bartholomew's Hospital may remember my directing attention in the early part of this year to a case in which I was called upon to operate for the relief of strangulated inguinal hernia on a man on whom I had performed Wützer's operation for the radical cure of hernia three years previously, with apparent success. In this case, as in most others, the inverted plug of integument had come down, and a fibrous constriction of the peritoneal sac remained, which proved the seat of the stricture. I believe this fibrous constriction, in part at least, to have been due to the changes consequent upon the passage of the needle in Wützer's operation, and it had served to keep up the intestine very fairly for above two years and a half. It then yielded, and became the seat of stricture to a protrusion somewhat larger than had ever been before.

"I saw last year a boy on whom I had performed Wützer's operation six months previously. In his case, too, the intestine had come down again; the protrusion was of larger size, and the adaptation of a truss was difficult.

"Under these circumstances, I was induced to ask my friend, Mr. Kingdon, Surgeon to the City of London Truss Society, the results of his experience amongst those who apply to him for trusses, and he favoured me with the subjoined list of cases, which I inclose, with all the details, that any of the surgeons who operated may be able to verify or to contradict their accuracy.

"There can be only one opinion respecting operative surgery. Its only claim is based upon the permanent benefit which it can effect; and if experience proves that any one operation fails, even at a remote period, in its object, it is the duty of those acquainted with the failures to make the facts public, in order that a proper value may be attached to the proceeding.

"Case 1. Thomas B., aged thirty-five, of Welling, Kent. Ruptured for the first time at the age of thirty-two. Wützer's operation for the radical cure of hernia was performed in St. Bartholomew's Hospital, by Mr. Coote in 1858. Case reported as cured. In 1859 he applied for a truss, at the City of London Truss Society, with oblique hernia on both sides.

"Case 2. Nathaniel J., aged forty-five, of Phœnix Terrace, an engineer. Operated on in the same way as Case 1, by Mr. Coote, three years and a half ago. He continued well until a few months back. Has applied for a truss, having now hernia on both sides.

"Case 3. Thomas P., aged thirty, West India Road, Limehouse. He was first ruptured at the age of twenty-eight. The operation for the radical cure was performed by Mr. Corner, of the Poplar Hospital. He applied for a truss, having a scrotal hernia, in 1859.

"Case 4. John H., aged fifty-one, of Churchway, Somerstown. The operation for the radical cure was performed by Mr. Erichsen. He applied for a truss in 1859.

"Case 5. James A., aged fifteen, applied for a protecting truss, having just risen from bed after the performance of Wützer's operation. No immediate protrusion.

"Case 6. John T., aged forty-four, of Duke Street, Westminster Road, bedstead maker. Ruptured at the age of twenty-four. He came for a truss at the Society's house in 1860, being at that time forty-four years of age. He had undergone the operation for the removal of a diseased testis nine months before, by Mr. Coulson, at St. Mary's Hospital. Had hoped that the hernia was cured at the same time. He now (1860) suffers from a large scrotal hernia.

"Case 7. George C., aged forty-three, of the Broadway, Westminster, gasfitter's labourer. The operation for the radical cure was performed by Mr. Lee, of King's College. He applied for a truss on Oct. 5th, 1860.

"'I thought,' continues Mr. Kingdon (writing to me), 'and still believe, that I had more of your cases; but the foregoing are those which I can vouch for at present. Two, and I think a third, of Mr. Hutchinson's have come before me; one also of Mr. Wood's.'"

34. *Venous Hemorrhage.*—This subject has been very fully discussed by LANGENBECK in the first part of the *Archiv für Klinische Chirurgie.* The causes of difficulty or impossibility of arresting venous hemorrhage are, adhesion of the vein to the neighboring parts; pressure of the column of blood; and regurgitation of the blood. The first mentioned cause of difficulty is especially observed in trephining and in operations for necrosis; and in operations involving the veins of the neck and upper part of the thorax, as the subclavian, external or internal jugular, and axillary. Death may follow wounds of the external jugular vein, even above the point at which it penetrates the cervical fascia. The division of the axillary vein gives rise to unimportant hemorrhage in comparison with the external jugular, because it is but loosely connected with the fascia, while the external jugular is stretched by the fibres of the platysma myoides. Hemorrhage in wounds of the large veins may take place from the peripheric as well as from the central end. From the former, the blood flows in a steady stream; from the latter, the bleeding occurs during expiration, or during crying or coughing. Gaping incised wounds of the common jugular vein are fatal, unless immediate surgical aid be at hand. In penetrating wounds of this vein, the blood poured into the sheath may act as a compressing agent and close the orifice; but, if the blood escape into the cellular tissue, a troublesome sanguineous tumour is the result. Suppuration, secondary hemorrhage, and pyæmia may also follow punctured wounds of the jugular. Gunshot wounds of the large internal veins, or of the internal jugular, may not give rise to serious bleeding at the time, but are liable to be followed by secondary hemorrhage and thrombus.

Ligature of the corresponding artery is a sure means of arresting hemorrhage from a large vein, as the internal or the external jugular, after compression has been tried and has failed. Hemorrhage from wounds of the external iliac and femoral veins is more troublesome than might be expected from their small size and their distance from the heart. In wounds of the internal jugular, the pressure of the column of blood may be diverted to the vein of the opposite side by compression, in consequence of the free communication through the cranial sinuses; while, in the veins of the lower limb, this cannot be done. The iliac and pleural veins bleed usually from the peripheric end; hence J. Roux has advised ligature of the femoral vein in disarticulation of the thigh. Langenbeck, however, has in six cases observed no hemorrhage from the femoral vein in this operation, although he did not tie the vessel—a proceeding which he considers dangerous.

Dangerous, and even unmanageable venous hemorrhage may be produced by compression of a venous trunk above a wound, by the pressure of a tumour, by obliteration of a vein, or by interference with the pulmonary circulation. In the leg, for instance, venous hemorrhage is not unfrequently produced by the bursting of varicose veins. Hemorrhage from wounded veins, the trunks of which are compressed by tumours, demand ligature of the corresponding artery. Very obstinate also are the capillary venous hemorrhages, dependent on rupture of the small vessels and compression of the venous trunks. Spontaneous hemorrhage from tumours is generally venous or capillary, when it arises from pressure of the veins returning the blood. It occurs most readily in very vascular growths, permeated by enlarged sinuous veins, or constricted at the base by the neighbouring parts. Hemorrhage may occur from internal piles, when they are extruded in defecation and compressed by the sphincter ani; or from polypi of the fauces or of the uterus, when the bloodvessels in them have a cavernous structure. Cirrhosis of the liver sometimes, but very rarely, produces vesical hæmaturia through obstruction of the inferior cava; generally, however, the gradual development of the cirrhosis allows the establishment of a compensatory circulation through the anastomosing veins.

The fact that the venous hemorrhage in great operations varies with the

respiratory act, has led Langenbeck to desire the patient in such cases to breathe deeply; that is, of course, unless he be under the influence of an anæsthetic. But in wounds of the large veins of the neck, deep inspiration must be avoided, on account of the danger of the entrance of air into the veins. In tracheotomy, the hemorrhage from the thyroid veins is arrested by the inspiration, which takes place as soon as the air-passages are opened. Styptics are regarded by Langenbeck as of very little use in obstinate venous hemorrhage; while, in ordinary cases, the bleeding ceases spontaneously. Obstinate bleeding from the smaller veins is best arrested by the actual cautery. In hemorrhage from the larger veins, compression with the finger must be first tried; a slight amount of pressure may be sufficient. When a vein of one of the limbs is cut across, compression of the distal end is generally sufficient; but, if this fail, the proximal end must also be compressed. In wounds of the external or of the common jugular, the compression must be first applied to the central or proximal end, on account of the danger of entrance of air. In hemorrhage from the larger veins, digital compression is only a temporary means, and must be replaced by a bandage. The object of the bandage is to close the wound without impeding the circulation; and for this purpose Langenbeck prefers strips of sticking plaster, where the edges of the wound can be brought together. When the vein lies deep, and the edges of the integument will not come together, he advises that a piece of lint smeared with cerate be laid on the vessel, and that compression be made by filling the wound with charpie. If a ligature must be applied, it is generally sufficient to tie the distal end; but the proximal end must also be tied, if large branches open into it. In the removal of tumours, when it is an object to avoid loss of blood, the vein may be ligatured in two places previous to the operation, and cut through; the ligatures are removed after the operation. Ligature of veins is, however, to be avoided as much as possible, especially in large hospitals, on account of its liability to be followed by thrombus and pyæmia. The vein may again become pervious, by the removal of the thrombus formed in it when ligatured.

On ligature of the arteries in venous hemorrhage, Langenbeck remarks that the dread of inducing gangrene has probably restrained surgeons from having recourse to this proceeding. But, while ligature of a large artery may be followed by capillary anæmia and functional disturbance of the part supplied, which may be gradually removed as the circulation becomes re-established; ligature of a vein is likely to produce venous capillary hyperæmia and serous exudation, and even inflammation of the vessel. Ligature of both the artery and the vein, however, appears less likely to produce mischief than when only one vessel is tied; at least, Langenbeck has in two cases tied both the carotid artery and the common jugular vein, and in neither were there any symptoms of importance referable to disturbance of the cerebral circulation. He refers also to a case in which hemorrhage of the femoral vein was arrested by ligature of the corresponding artery; and to an instance of popliteal aneurism mentioned by Crisp, in which hemorrhage from the wounded popliteal vein ceased when the artery was tied. He is evidently reluctant to tie a vein when such a proceeding can possibly be avoided.—*British Medical Journal*, June 15th, from *Schmidt's Jahrbücher*, Bd. 109.

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35. *Treatment of Bubo by Injection.*—Dr. Pizzorno strongly recommends the following mode of treating bubo, which he has pursued in more than 500 cases with great advantage and no ill effects: When suppuration cannot be prevented, the bubo should be opened sufficiently only to discharge the pus and to admit the point of a small glass syringe. A solution of corrosive sublimate (two grains to three ounces) is then to be thrown in with force two or three times in succession, and after the sac of the abscess is thus well washed out, a little of the solution is still to be left within it, the aperture being then covered with some shreds of lint and compresses moistened in the solution. Pressure is also to be made with a long and broad bandage. Sometimes at the end of the first twenty-four hours suppuration has ceased, and the external application alone has to be continued. When this is not the case the injection has to be repeated every twenty-four hours until the suppuration ceases. Upon an average about seven-

teen days are required to obtain a complete cure of the bubo. In indolent bubo, when frictions with extract of cicuta and blue ointment, a few doses of calomel, and compression by bandages do not succeed, the author applies a blister and dresses it with a weak solution of corrosive sublimate.—*Med. Times and Gaz.*, August 17, 1861, from *Omodei's Annali*, Feb.

36. *Gunshot Wound of the Heart.*—Dr. R. ADAMS read before the Surgical Society of Ireland an interesting case in which the left ventricle of the heart was perforated through and through, by a gunshot wound made by a leaden slug the size of a small swanshot. The subject of it was a man æt. 43, who, whilst passing along the street, accidentally received the contents of a gun. The man, when shot, staggered back, but soon recovered himself, and ran unassisted into a neighbouring surgeon's shop, complaining of weakness, and having the aspect of extreme exhaustion. He was speedily taken to the Richmond Hospital, Dublin. When he arrived there, he was pale, pulseless, and gasping. He was offered a wineglassful of brandy, which he drank well, then expressed a wish to be raised from the horizontal posture and be placed sitting, which was done accordingly, but in five minutes he expired, having lived about one-half hour after he had been wounded.

On post-mortem examination the next day, it was found that the slug had entered the skin behind and below the level of the nipple of the left breast, struck the fifth rib, and then passed through the intercostal space between the fifth and sixth rib, traversed the pericardium and left ventricle of the heart. Two ecchymosed spots on the outside of the pericardium showed the places of entrance and exit of the shots. The pericardium was distended with coagulated blood, which was seen to ooze out through the small apertures made by the fatal shot. The bag of the pericardium was then slit up, the mass of black coagulated blood sponged out, and two round holes, capable of permitting a goose-quill to pass through, was noticed to perforate the left ventricle. The anterior opening, or that of exit, made by the slug in the left ventricle, was about the size of a split pea, rounded in form, but presented somewhat of a lacerated margin, and with an ecchymosed areola round it. The posterior opening, or that of entrance, was smaller, more regular in form, and scarcely any ecchymosis around its margin. When the relative position of the six perforations, which the fatal shot had made, was contemplated, and a probe passed from the external wound, it was plainly to be inferred that the most posterior wound in the skin behind the nipple of the left breast was the spot where the shot first entered, that the fifth rib was struck, and caused this shot to turn a little downwards, and that it thence passed from behind forwards, having entered the left ventricle of the heart posteriorly behind its left margin, about an inch and a half from its apex, and passed out in front close to the anterior edge of the septum ventriculorum. Looking at the relative position of these openings, it is difficult at first to conceive how a part of the left ventricle, so deeply situated in the thorax as the opening of entrance was, could have been thus wounded; we must under the circumstances only conclude, that at the critical moment the heart was stricken the whole organ must have been strongly rotated on its long axis from left to right, and the left margin of the heart thus become elevated. Search was made for the slug near to the junction of the diaphragm, with the base of the thorax anteriorly, but it could not be found. It is easy to imagine how the left lung could have escaped when the slug made its exit anteriorly, as this is the part of the heart uncovered by the lung, but difficult to conceive how so retired a portion of the left ventricle could have been thus penetrated and the left lung escaped, which appeared to have been the case, as no ecchymosed spot appeared on this organ, nor had any air escaped, as the lung at the part under examination was full and distended, concealing the pericardium.

It would appear that the slug in its course passed rather obliquely through the wall of the ventricle, and that the little canal which formed its trajet only communicated in about the middle of its course by a small lateral opening with the cavity of the left ventricle. The ultimate result was, however, the same as if the cavity of the left ventricle had been more directly traversed through and through at once. These two small perforations in the left ventricle of the heart

permitted the blood to ooze out gradually, and in the course of half an hour (the time the man lived after the injury) so much blood had exuded from the interior of the heart as was sufficient to exert such a pressure from without on the ventricles and auricles, so as to annihilate the heart's action, prevent its diastole, and cause death. In short, the external pressure of the effused blood had completely compressed the heart just in a similar manner as we occasionally see the lung compressed and its function annihilated by pus in empyema, by water in hydrothorax, or air in some forms of traumatic emphysema.—*Dublin Medical Press*, April 3, 1861.

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37. *Congenital Phymosis considered as a frequent Cause of Irritation of the Urinary Organs of Young Children, and the Importance of its early Recognition and Removal.*—Mr. Price, in a paper read before the Medical Society of London, stated that one of the most common deviations from the normal construction of the preputial and glandular portions of the penis, consisted in an undue prolongation of the prepuce, adhesion of the mucous membrane of this structure to the glans, and the retention of a quantity of secretion from the odoriferous glands around the corona glandis. Such a condition is termed phymosis and might be either congenital or induced. He intended, however, strictly to confine his remarks to the congenital form, and to show that such a malformation proves a very frequent cause of genito-urinary disturbance in male infants and boys. Congenital phymosis, although a very common condition, is one which does not always attract the notice of the surgeon, and lead him to the correct appreciation of the true cause of various urinary complaints in infant life. Sir Charles Bell, Mr. Guthrie, and other well known writers on Diseases of the Urinary Organs, had failed to allude to congenital phymosis as the cause of certain urethral and bladder affections in the young.

Latterly, however, greater consideration had been paid to the subject; but still it was one which had not attracted the attention it deserved.

The author then proceeded to detail the most common results which follow in infants and boys from congenital phymosis, and stated that they might be enumerated under five distinct heads:—

1st. A preputial obstruction to the flow of urine, and its more or less permanent retention.

2d. Constant irritation of the glans penis and urethra.

3d. Vesical irritation, accompanied by incontinence or retention of urine.

4th. Hypertrophy of the muscular coat of the bladder, associated with symptoms bearing a close analogy to those of catarrh and calculus vesicæ.

5th. Pain and swelling of the testicle.

Each one of these conditions was illustrated by a series of cases, which tended to prove that the abnormal conformation of the prepuce is a much more common cause of urinary and genital affections than is generally believed.

The author dwelt at some length on the great similitude between the symptoms caused by a tight congenital phymosis and those induced by the presence of a stone in the bladder, and quoted two instances to illustrate the serious mistake into which the surgeon might fall, by not obtaining a clear indication of the existence of a calculus prior to submitting a young patient, suffering from all the symptoms attending the presence of a stone, to the operation of lithotomy. With the usual description of congenital phymosis the author was disposed to find fault. It was generally stated, that "the unnatural narrowing of the prepuce, so that it cannot be readily retracted over the glans penis," constitutes the condition termed phymosis. True phymosis, however, consisted of something more than this; for besides the mere elongation of the foreskin, and its unnatural narrowing, there usually existed firm mechanical adhesion between the preputial mucous membrane and the glans penis, whereby the foreskin could not be retracted, and a quantity of the secretion of Tyson's glands became pent up without the least chance of escape. In fact, the author believed that it was this latter condition of parts which formed the chief or sole cause of genito-urinary disturbance. Such being the usual condition constituting congenital phymosis, it was evident that some of the operative measures generally advocated for the removal of the malformation are, to say the least, inefficient. It

had been suggested by more than one surgeon, that a mere division of the mucous membrane of the prepuce was sufficient, and to effect this, it was proposed to divide the structure subcutaneously, by passing a thin bladder-knife through the preputial orifice. Such a plan might suffice if the mucous membrane remained non-adherent to the glans; but in congenital phymosis of young children, the author believed that in the majority of instances such was not the case. Many cases were quoted, showing that an inefficient operation was only fraught with temporary relief, and that after the lapse of even a short period, the unnatural state of parts returned. The author stated, that as he was in the habit of performing the operation for phymosis somewhat differently to most surgeons he would briefly detail the method he employed. In young children he usually advised the exhibition of chloroform, as much pain was not only thereby avoided, but it was exceedingly difficult, otherwise, neatly to finish the operation, owing to the contortions into which the little patient generally twisted his body. The prepuce being put on the stretch, so as to remove the orifice as far from the glans penis as possible, the blunt-pointed blade of a pair of scissors is passed through the preputial orifice to the distance of about a quarter or half an inch towards the corona glandis, and the prepuce divided through its upper surface. This will allow the glans to be seen, covered, in all probability, especially if the phymosis be congenital, by the mucous membrane of the foreskin, which will be found more or less tightly adherent to the glans. Laying aside the scissors, this adhesion is to be separated by somewhat forcibly retracting the back portion of the prepuce, and by means of the thumb-nail breaking through such points of union as may be more than usually firm. Around the fossa in front of the corona glandis will generally be found small patches, or masses, of hardened and otherwise altered secretion of the odoriferous glands. These being removed, the scissors must be again employed, and such portions of the redundant skin and mucous membrane cut away as will allow of the glans penis, after the edges of the wound are united, remaining uncovered. In removing the necessary portions of integument, the author generally contrives, by shaping the incision somewhat obliquely from above downwards, to avoid wounding the artery of the frænum, whereby there is little risk of secondary hemorrhage and infiltration of the loose tissue with blood—a result which he has frequently known prove troublesome, and for a considerable time retard the healing process. If, however, it be desirable to loosen the connection of the mucous membrane with the glans at that part which is called the frænum, instead of dividing the little artery which runs in this direction, it is better to separate, with the scissors the union of the frænum with the glans immediately in the line of junction, by which means the course of the vessel will be avoided. Mr. Price, although some surgeons do not follow the practice, is in the habit of keeping the mucous and cutaneous surfaces in contact by sutures, unless the patient on whom the operation is performed be very young. Sutures of the very finest silk are to be preferred to those in ordinary use, and made either of the same material or of metal, as they cause less irritation, and do not need removal before their destruction has been effected, in the course of a few days, by reason of the contact of the healing or other discharge. To prevent the urine, for the first few hours after the operation, from excoriating the edges of the wound, it is advisable to smear them with oil or cerate. In performing such an operation less danger will be run of wounding the glans penis—a result which the author has known to occur when the bistoury has been used. It must be recollected, that to obtain complete relief from a congenital phymosis, a mere slitting up of the elongated prepuce will not generally suffice; but a certain portion of the mucous membrane and cutaneous tissues must be removed.—*Lancet*, May 4th, 1861.

38. *Coccygectomy.*—Prof. SIMPSON brought under notice of the Edinburgh Obstetrical Society (March 28, 1861), a patient in whom he had cut out a portion of the coccyx, and had thus cured her of a severe degree of coccyodynia. The case, which Dr. Simpson related at full length, had already been published in the *Medical Times and Gazette;* but he had asked the patient to show herself at the Society, in order that the members might satisfy themselves as to the

degree of relief that had been obtained.—The patient, on being interrogated by the Chairman, stated that she had suffered for many months from excruciating pains in the region of the coccyx, which never entirely left her, but were least distressing when she was standing or walking, whilst in other positions it was very intense. She could not sit on a chair with any degree of comfort, and only by resting on one hip at time. To lie on her back in bed was utterly impossible. She attributed the occurrence of the disease to exposure to cold whilst sitting on damp grass, watching clothes drying. She had got various remedies with a view to her relief; but the only lasting relief she had ever obtained was by means of an operation performed by Dr. Alexander Simpson (isolation of the coccyx, as that gentleman explained), which had left her free from pain for about a fortnight. The pain having returned, however, and become of a stounding character, she was sent into the hospital to have a piece of the bone removed; and, since the operation had been performed, she had found herself perfectly well, and was now able to do all her ordinary work as a laundress, for which she had been utterly incapacitated. So far as the coccyx was concerned, she felt no difference between its present condition and that in which it had been in her earlier years.

The Chairman thought the woman's statements very satisfactory.

Dr. Simpson begged to add, in reference to the disease, that he thought different sets of muscles were involved in different cases; and, to produce a cure, it was in most cases necessary only to divide the muscles affected, or their tendons. He had seen only this and one other case where isolation of the coccyx from the muscular and tendinous attachments had failed in effecting a cure. In some cases the operation required to be repeated, in consequence, probably, of some of the fibres of the implicated muscles not having been cut through. He had lately had under his care a lady from America, in whom a first operation afforded partial, and a second complete relief.—*Edinburgh Med. Jl.*, July, 1861.

OPHTHALMOLOGY.

39. *Treatment of Acute Glaucoma by Iridectomy.*—The following case occurred at the Moorfield's Ophthalmic Hospital, under the care of Mr. Dixon.

"Mrs. P., aged 46, was admitted December 27th. She was a thin, moderately florid woman, not subject to gout or rheumatism. Twelve days before admission, she had severe pain in the right eye, which came on suddenly at night. Next morning, she found that she could scarcely see with it; and the pain, still severe, had continued up to the time when Mr. Dixon saw her. Blisters and various applications had been used, but without any benefit. When admitted, she could with the affected eye only just distinguish light from darkness, and could not count fingers held up between the eye and the light. The sclerotic was dusky, leaden-coloured, and crossed by enlarged veins. The pupil was dilated moderately, and fixed. The fundus, as seen through the pupil, looked dull and yellow. The cornea had lost its brilliancy. Atropine did not produce any effect on the size of the pupil; whilst in the other eye the pupil readily dilated under its use. The ophthalmoscope was used; but a mere red reflex from the fundus was seen. The woman would not submit to the operation of iridectomy, which was at once proposed, until the 31st, the fourteenth day from the attack, when iridectomy was performed. The piece of iris removed was from the upper part. No chloroform was given. The pain was at once relieved.

"Four days after the operation, she had had no return of pain, and could read No. 10 of Jäger's test-types. There was a minute fistula in the cornea, to which Mr. Dixon applied nitrate of silver. She gradually improved, and was soon able to read small type.

"On January 25th, the left eye became suddenly very painful. The pain was in the globe; it was very intense, and was accompanied by vomiting. She was not seen by Mr. Dixon until the 28th. He then found that the eye was in exactly the same state as the right had been. She could only just count fingers. The

pupil was dilated and fixed; and the fundus could not be illuminated by the ophthalmoscope. Iridectomy was at once performed, and with the same amount of success as attended the operation on the other eye. She can now see well with both eyes, and can read pearl type without the aid of glasses.

"Mr. Dixon remarked that, in this case, the disease had been of the exact form to the relief of which iridectomy is best suited; viz. *acute* glaucoma. In the chronic cases, as far as he had seen, little or no benefit followed the operation, but rather an increase of the irritation. It is interesting to note that the attack in the left eye occurred whilst the right was going on satisfactorily. The ophthalmoscope had not been used to examine the sound eye, and therefore no blame was traceable to it, as having re-excited the disease in it. The occurrence of glaucoma in the second eye a few weeks after it has attacked the first is quite in accordance with what is frequently observed; and hence, as both eyes generally suffer, the value of iridectomy is much increased."—*British Med. Journ.*, March 16, 1861.

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40. *Encysted Abscess in the Vitreous Humour.*—Mr. J. G. HILDIGE records (*Med. Times and Gaz.*) a case of this rare affection. The subject of it was, a man 58 years of age, sallow, unhealthy looking, but who says he has always enjoyed good health, who had been suffering from inflammation of the eyeball for several weeks. The eyelids of the affected eye were very much inflamed and swollen, the folds of the integument being completely obliterated, the eye itself was intensely chemosed and projecting from the orbit, the cornea was opaque in its whole extent, and the severe lancinating pains in orbit and temple had almost wholly banished sleep for nearly a fortnight. The right eye was sympathetically affected, and he was unable to open it, on account of the very great sensibility to light. He stated that about five weeks previously he had caught cold in his eye while out walking, for which he had been treated at the dispensary in the neighbourhood. On his way to the dispensary one very cold morning, his symptoms became much aggravated, and on the following day intense pain in the eyeball set in, which was speedily followed by a total loss of vision. The pain was continuous, but was subject to exacerbations, which became intense at night, lasting until four or five o'clock in the morning, when it slightly abated. As the patient had been salivated, leeched, blistered, etc., without effect, and as he was wasted to the last degree, further medical treatment was out of the question; Mr. H. therefore, enucleated the eyeball and the following was the result of the examination of the eye made by Drs. Cruise and Curran:—

"The form of the globe was considerably changed, its antero-posterior measurement being much greater than in the normal condition; its transverse diameter was also slightly increased. The cornea was perfectly opaque, and its curve much greater than natural. On making a transverse section of the eyeball, a perfectly circumscribed abscess, about the size of a small pistol-bullet, inclosed in a membranous sac, and containing laudable pus, was observed in the centre of the vitreous humour. It was perfectly free, posteriorly and laterally, but anteriorly it was connected by fibrous bands to the ciliary body. The portion of the vitreous humour immediately surrounding the abscess was opaque, but no pus-globules were detected in it. The iris was attached to the cornea in its whole extent, the pupil was filled up with lymph, and the lens and its capsule were perfectly opaque. From the very great congestion of the choroid, I fully expected there would have been exudation found between it and the retina, and partial detachment, at all events of the latter, from the choroid, but neither one nor the other of these results was present. The increased sensibility and irritation of the right eye rapidly diminished after the operation, and the patient can now distinguish the surrounding objects with it.

"On referring to Professor Arlt's work on 'Diseases of the Eye,' I find the following remarks in the chapter on Exudations into the Vitreous Humour:—'Post-mortem examinations have proved beyond doubt that results occur in the vitreous humour, which can only be considered as the products of inflammation. It is, however, very probable that these products are not generated in the vitreous humour itself by inflammation of its substance, or of its enveloping mem-

brane, but are carried into it from the surrounding tissues. For, neither the vitreous body, nor the hyaloid membrane, is furnished with vessels or nerves, without which inflammation cannot take place, and such products are never met with in the vitreous body, without inflammation of the choroid being present. The term 'Hyalitis' owes its existence, not to the observation of symptoms, which can only be referred to inflammation of the vitreous body, but to the attempt to create a train of phenomena for each tissue of the eye, which will convey the idea of inflammation of it, in order that no gap may be left in the systematic representation of the diseases of the eye. The only real foundation for the disease termed Hyalitis, is to be found in the occurrence of inflammatory products in the vitreous humour, which, however, can and must be explained in a very different manner. These products are the result of inflammation of the ciliary body or choroid, and are carried into the interior of the vitreous humour, in the same manner as the nutritious plasma in the normal condition.'"

MIDWIFERY.

41. *On Superfecundation and Superfœtation.*—Dr. Kussmaul distinguishes superfecundation from superfœtation, and he proposes the word "superimpregnation," in order to express a new conception, which has happened during the course of pregnancy. The author discusses the following four questions:—

Is it possible that during pregnancy new ovules can become mature and detach themselves from the ovary?

Is the state of pregnancy of a matrix simple, or of one of the halves of a double one, an absolute obstacle to a second fecundation?

What is the value of the facts recognized up to the present time relating to the result of superfœtation in a simple or double matrix?

The following are the results of researches to which Dr. Kussmaul devoted himself regarding this argument:—

1. We must distinguish superfecundation from superfœtation, and unite both these phenomena under the name of superimpregnation.

2. There is superfecundation, if, in consequence of several intercourses of the sexes, there is fertilization of several ovules which have been matured during the same period of ovulation. This phenomenon is proved in the horse, and exists in all probability also in man.

3. Superfœtation would take place if an ovule of the second or any other period during pregnancy could be fertilized, but until now the possibility of this fertilization has not been established with certainty in woman, because it is proved that ovulation exists in women generally during pregnancy, and that all cases considered until now as superfœtations are open to other interpretation.

4. We possess up to the present time no authenticated case of any individual superfœtation in extra-uterine pregnancy. Supposed cases of the sexes are explained by the fecundation of two ovules from the same period of ovulation, which are developed on different points in consequence of a new fecundation after the death of the embryo which was developed outside of the uterine cavity.

5. We possess no positive evidence that a woman whose matrix did contain a dead fœtus was able to conceive.

6. Neither the membrane nor the mucus which obstruct the cavity of the neck of the uterus, could be considered as an absolute obstacle to superfœtation in a single or double matrix.

7. The only obstacle which opposes the passage of the seminal liquor in a simple matrix in a state of gestation is the embryo itself, when it fills up the uterine cavity and closes the openings of the oviduct. In a double matrix nothing can hinder the passage of the seminal liquor in that part which has not been impregnated during the time of pregnancy.

8. The most probable facts with regard to superfœtation are those relating to the birth of twins at long intervals; but these facts can be explained as the

birth of twins, one of which was too forward, the other late; that is to say, the one took more time than the other for complete development.—*Dublin Med. Press*, Aug. 25, 1861, from *Presse Méd. Belge.*

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42. *Modes of Termination of Extra-Uterine Pregnancy.*—Dr. MATTEI relates the details of a case of extra-uterine gestation which occurred in his practice, and states the following results of his examination of the details of 100 recorded cases, references to most of which he furnishes. Of the 100 women, 12 died accidentally, after having retained the result of pregnancy during several years; 5 died in consequence of the accidents of pregnancy without the cyst having opened externally. In 37 instances an opening was made into the cyst through the abdominal walls—viz., spontaneously in 21 (20 of these women recovering), in 7 with the concurrence of art all recovering, and in 9 by gastrotomy, all recovering. In 8 cases the opening took place into the bladder, 2 of the women dying. In 7, into the vagina, the opening being spontaneous and fatal in 2, and artificial, with 4 recoveries, in 5. In 31 cases the opening took place into the intestinal canal, either with the intervention of art or not, and of these 12 recovered and 19 died. The author's conclusions are: 1. Old extra-uterine pregnancies allow of life being prolonged for many years, but such cases are rare (12 per cent.), and are especially met with when the fœtus dies at an early period, and when the cyst has remained of small size. 2. Extra-uterine pregnancy may (5 per cent.) become the cause of death without the cyst opening externally or into a mucous membrane. 3. Whether the cyst gives rise to accidents prior to or subsequent to its being opened, it is not easily tolerated by the economy, and is expelled in more than a third of the cases in from one to two years, in a sixth between two and five years, and so on in decreasing progression. 4. Of all the modes of termination that by an opening through the abdominal walls, whether spontaneous or artificial, has been the most frequent (38 per cent.) and the most favourable, serious inflammatory accidents and resorptions scarcely ever occurring. 5. Opening into the bladder is rare (8 per cent.) and less often followed by recovery. 6. That into the vagina is somewhat rarer still (7 per cent.), and what is surprising is that it is much oftener fatal, especially when art does not intervene. 7. Opening into the intestine is of frequent occurrence (30 per cent.), and is the most fatal of all terminations, three out of five women dying. 8. It is imprudent to treat these cases by expectation, unless, indeed, the cyst is small; and even then it should be at once evacuated, when there are symptoms of an opening taking place, such as sudden change in the health, fever, rapid emaciation, the disappearance of fluid from the cyst, local signs of reaction, and especially diarrhœa. 9. When there is time to wait, it is best to open the cyst at different stages, caustics being often preferable to cutting instruments. 10. If we have the choice of the place where the aperture is to be made, this should be in the abdominal parietes; but if Nature has commenced the eliminating process at any point, it is there we must act, and that as promptly as possible, especially if this be the intestine.—*Med. Times and Gaz.*, August 17, 1861, from *Gazette des Hôpitaux*, 1860, No. 110.

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43. *Labour Twice Prematurely Induced on a Dwarf with Distorted Pelvis.*—Dr. E. A. KIRBY read before the Royal Med. and Chirurg. Soc. (Feb. 26th, 1861) an account of a case of this kind. The subject of it was a dwarf of singularly small stature and ill development. In infancy she had rickets, and her osseous system was considerably distorted by the inroads of that disease; but it appeared that some other members of the same family were also very diminutive in stature and development, who nevertheless were exceedingly well proportioned, and apparently free from disease. Therefore, apart from the distortion consequent upon the rickets, she was no doubt a true dwarf. When first seen by the author, in the early part of 1858, she was twenty-seven years of age, had been married twelve months, and had completed her thirty weeks of uterine gestation. She presented a strumous aspect, and her general health was indifferent. On a careful examination, the spinal column and pelvis were found to be greatly distorted. The former from the last cervical to the union of the last dorsal and first lumbar vertebræ, measured fourteen inches, and presented

three curvatures, two lateral and one angular forwards. This latter curvature involved the whole of the lumbar and the last dorsal vertebræ, the bodies of which were thrust forwards, downwards, and to the left. The pelvis was contracted in all its dimensions, and slightly flattened from before backwards; the promontory of the sacrum and last dorsal vertebra projecting forwards to the pubis, narrowing the antero-posterior diameter of the upper outlet to rather less than two inches and a half. Natural labour under these circumstances being impossible, the choice of operations lay between craniotomy and the induction of premature labour. The latter was determined upon, the time chosen being the thirty-second week of gestation, but it was delayed for a week owing to the patient suffering from bronchitis, and was only completed at about the thirty-fourth. Induction of the labour was commenced on January 30, and she was delivered on February 7. On rupture of the membranes, the elbow was found in the vagina, and the shoulder resting on the brim of the pelvis, the child lying transversely. This embarrassing presentation was converted into a footling by turning, and in about twenty minutes the little patient was delivered of a living child. Both progressed favourably, and in a few weeks the woman returned to her usual avocations perfectly well. In this case about two ounces and a quarter of ergot was employed with the most useful effect, and without occasioning the least bad symptom in mother or child. A few months later this little dwarf, nothing daunted by her past experience, again became *enceinte*, and having completed her seventh month, applied to the author, who again induced premature labour. This time, however, owing to the lateness of her application, and to the labour proceeding more slowly than in the first case, the delivery was not effected until she had completed her eighth month of uterine gestation, two weeks later than in the previous case, and the fœtal head had attained a degree of development incompatible with its safe passage through so contracted an outlet. The child was therefore lost, but the parent recovered as rapidly as before, and she has not since been pregnant. Her first child has by this time grown nearly as tall as herself. The means employed to induce both labours were fully described, and present several points of interest. On remarking on these cases, the author laid much stress on the value of the operation, and its great superiority to craniotomy, in which operation 1 in 5 mothers dies, while in induction of premature labour 1 only in 50 dies; and half the children are saved. The success of the latter operation he thought greatly depended on keeping the membranes entire until the os uteri was fully dilated, and he reprobated the rupture of them as a means of inducing labour, it being most dangerous to the life of the child, and seldom if ever required.

The paper was accompanied by excellent stereoscopic photographs, taken by Dr. Wright about a fortnight before the last delivery; and, although not the first application of photography to medical science, this is probably the first paper that has been read to the society so illustrated.

Dr. Greenhalgh had had considerable experience in connection with deformity of the female pelvis. He had had occasion to perform Cæsarean section twice, and to induce premature labour five times. He could support Dr. Kirby's opinion as to the value of ergot, as he had used it in the five cases of premature labour he mentioned, and with the best effects. He had never seen, and did not believe that any injurious effects to the child were ever produced by the drug. In cases in which the child was born dead after the administration of large doses of ergot it was reasonable to suppose that death was caused by the long and difficult labour which had rendered the use of the ergot necessary for the accomplishment of delivery. He wished to ask Dr. Kirby what means were taken to insure exactness in the measurements he had detailed, and if chloroform was used, which he remarked was a very important aid? He (Dr. Greenhalgh) remembered a case which occurred at the Middlesex Hospital, which bore on the subject of discussion. A woman had been sent to the hospital, in order that Cæsarean section might be performed. The case was carefully entered into by Dr. Hugh Lee and Dr. Merriman's father, and it was agreed that Cæsarean section was the only method by which the child could be delivered. Arrangements were made for the operation, and the instruments were got together; and when all was ready, the child was born alive. Mr. Greenhalgh dwelt on the

great difficulty in arriving at an accurate estimate as to the diameter of the pelvis; and had frequently found that very able men differed considerably in the results they had arrived at by examination of the same case. He was asked lately to perform Cæsarean section, but not considering the case calling urgently for immediate delivery by such a severe procedure, he waited several days; he then succeeded in delivering the patient by the aid of the long forceps. The child was putrid, and had evidently been dead some time.

The author stated that he had obtained his measurements by the aid of the finger, and that Mr. Foster, who saw the case with him, had by the same means formed the same estimate. He considered that there were circumstances attending the second case, which confirmed the opinions he had then formed. The labour had in this instance been delayed two weeks longer. The breadth of the head after delivery was only two and a half inches.

Dr. MERRIMAN wished to ask the author why the injection of warm water was not used as a means of exciting premature labour?

Dr. WYNN WILLIAMS stated that there were cases on record, in which, during the attempt to inject the uterus with warm water, air had entered the veins, and instant death had resulted.—*Med. Times and Gaz.*, March 9, 1861.

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44. *A Practical Inquiry whether the Ergot of Rye, when administered to the Mother during Labour, is Dangerous or not to the Life of the Child?*—Dr. R. UVEDALE WEST read a very valuable paper under this title before the Obstetrical Society of London (July 3, 1861).

In December, 1855, the author published the particulars and results of an inquiry similar to the present—to the effect that, in a series of 69 cases in which he had administered the ergot of rye on a gross total of 278 labours, 9 children were stillborn, viz., 2 putrid at birth; 2 born after labours which were preceded and accompanied by hemorrhage; 1 born footling, with hydrocephalic head, and consequent fatal compression of funis; 1 in which there was evidence of latent compression of funis; 1 a difficult primiparous forceps delivery; 1 a difficult vectis delivery, the mother seriously ill from excessive œdema; and 1 born dead without any assignable cause. In consequence of certain criticisms on this paper which were published in France, accounting variously for the results, and challenging the author to continue the inquiry on the same plan, one of the critics, Dr. Danyau, on the part of the Imperial Academy of Medicine, maintaining that, unfavourable though the results appeared to be to him, yet a similar report on a like number of cases would probably be more unfavourable still, Dr. West continued to tabulate his ergot cases as he had begun, with the results which he now begged to lay before the Society:—Between December 23, 1855, and June 22, in the present year, on a gross total of 734 labours attended by the author, the ergot was given in 172 cases, including 1 case of twins, so that there were 173 children born under the effects of ergot of rye. Of these, only 5 were stillborn from all causes—viz., 3 putrid at birth; 1 with placenta prævia and profuse hemorrhage—premature; and 1 with prolapsed funis, detected an hour after the ergot was given, the operation of turning, which was then immediately performed, having been too late to save the child's life. The author urged that, in reply to Dr. Danyau's precise challenge, he might fairly have rested contented with the results of the first 69 cases of this second series, in which there was not a single stillbirth from any cause; but he considered that that fact, when compared with the unfavourable result of 9 stillbirths in the preceding series of 69 ergot cases, proved only that 69 was too small a number to form a correct conclusion from, and therefore he went on with the inquiry until it was spread over a gross total of 1013 labours, on which number the ergot was given in 241 cases. Of that number of 241 ergot cases, including 242 children born, there were, adding the 9 of the first series to the 5 of the second, 14 stillbirths from all causes—viz., 5 putrid at birth; 1 footling; 1 prolapsed funis; 3 hemorrhage during labour from placenta prævia; 2 difficult instrumental deliveries; 1 suspected latent compression of the funis; 1 cause not manifest. On the whole number of births—1013 labours, and 1029 children born, including the series without ergot as well as that with ergot—there were 50 stillbirths, of which 5 were born dead without any manifest cause. So that, inasmuch as only 1 of

those 5 was born under the influence of ergot, that medicine having been given in the greater proportion of 1 in 4¾ of the whole number of labours, the author thought there was no sufficient evidence in the facts he had accumulated to justify the doctrine that ergot of rye was dangerous to the life of the child. As to the mother, the author found the following results on a retrospect of the gross number of 1013 labours—viz., 7 deaths within the lying-in month, of which one occurred with a patient who had had ergot of rye; 18 cases of incarcerated placenta, of which 5 were after ergot of rye; 25 instances of post-partum hemorrhage, of which 5 were after ergot of rye; 30 cases of puerperal disease or disorder, of which 9 were after ergot of rye. So that he was led to the conclusion that ergot has little or no influence in either causing or preventing parturient or puerperal accidents or diseases. At the same time he wished to remark, that probably in proportion as ergot might improve uterine action, certain accidents which depended on deficient uterine action, such as post-partum hemorrhage and after-pains, might be controlled by ergot of rye. The author concluded his paper with the observation that the tables before the Society appeared to prove that it was immaterial in what stage of the labour the medicine was given; whether the os uteri was rigid or supple; whether the liquor amnii was or was not evacuated; or whether the mother was multiparous or primiparous; but that it was essential that actual labour should be present, as well as that the accoucheur should be competent to meet any emergency that might arise, just the same as when ergot has not been given; that ergot was useful wherever it was desirable to improve uterine action; and that it could be dangerous only where uterine action would be dangerous, as, for example, in a case of arm presentation after the liquor amnii was evacuated, as then it would make turning more difficult.—*Med. Times and Gaz.*, July 20, 1861.

45. *Numerous Cæsarean Operations by one Practitioner.*—Dr. Winckel, of Gummersbach, near Cologne, practising among a rural population living in a very wretched hygienic condition, and very subject to osteomalacia, has had occasion during nineteen years to perform the Cæsarean operation, on account of deformed pelvis, *thirteen* times, the deformity arising from osteomalacia in eight, and from rickets in five instances. In four of the cases gastrotomy was required only, as, in consequence of rupture of the uterus, the children lay in the cavity of the abdomen. This occurred twice in one woman, who had already once before undergone the operation. Of these three women two recovered; and, in fact, the issue of the operations must be regarded as favourable, seeing that six of the women lived, one of them having been operated upon three times, and therefore giving a proportion of eight recoveries to five deaths. After the operations, which were performed under chloroform, opium was given, and no inflammatory symptoms ensued.—*Med. Times and Gaz.*, July 6, 1861, from *Monatsschrift für die Geburtskunde*, vol. xvi.

46. *Simple Treatment of Lacerated Perinæum.*—M. Hervieux says, that he has in several cases successfully treated laceration of the perinæum in females by the retention, in contact with the part, of a sponge dipped in a solution of chloride of zinc, in the proportion of one part to from ten to fifteen parts of water. The sponge is renewed four or five times a day, and is held in its place by a piece of waxed cloth, and a few turns of a bandage. The sponge, M. Hervieux says, imbibes the secretions, while the chlorine exerts on them a neutralizing and solvent action. Reparation is thus enabled to take place gradually; and the ultimate result is complete cicatrization. Two cases are recorded in illustration.—*L'Union Médicale*, May 16th.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

47. *Poisoning by Strychnia applied to the Punctum Lachrymalia.*—Dr. Schuler relates the following interesting case which occurred in Langenbeck's prac-

tice. In a case of amaurosis, in a man fifty years of age, about the twelfth of a grain (less than five milligrammes) of pure strychnia was introduced by means of an ear-pick into the *punctum lachrymale;* but as during this manipulation a portion of the powder was lost, about three milligrammes only entered the punctum. Three or four minutes had not elapsed when the patient's face became livid, and he was seized with spastic yawnings and vertigo. Free admission of air and cold aspersions were had recourse to, and "lavements" (!) were administered. The symptoms continued to increase, as shown by loss of speech and pulse, convulsive respiration, and violent tetanic shocks. Death seemed inevitable, when the severity of the symptoms abated, and after a copious evacuation of urine and feces, all had passed off in less than half an hour. From this fact it is evident that death might be rapidly caused by depositing in the corner of the interior of the eye from 5 to 15 centigrammes of strychnia or salt of strychnia; and supposing the remaining adherent powder to have been cleaned away, the detection of the cause of death might become a matter of extreme difficulty.—*Med. Times and Gaz.*, July 20, from *Gazette Médicale* de Paris, No. 6.

48. *Influence of Lead Poisoning on the Fœtus.*—M. C. PAUL, in continuation of former researches (see No. of this Journal for Oct. 1860, p. 567), relates two additional cases, showing the great liability of women to abortion who have to work amid a saturnine atmosphere, as in polishing type and the like. He has now examined 89 individuals with reference to this point, and of these 31 have become pregnant while exposed to the action of lead. These women furnished 141 pregnancies; of these 82 proved abortions, in 4 labour was premature, in 5 the children were born dead. Of the children born alive 20 died during the first year, 18 during the second, 7 during the third, and 1 still later. Fourteen of the children are still living, 10 only having passed their third year.—*Med. Times and Gaz.*, July 20, from *Gaz. Méd.*, No. 10.

49. *Overdose of Croton Oil—Recovery.*—Dr. J. BRYDON relates (*Edinburgh Med. Journal*, Aug. 1861) the case of a girl, 19 years of age, who took by mistake a teaspoonful of a liniment composed of equal parts of croton and olive oil. When seen by Dr. B. about half an hour afterwards, complained of an intense burning sensation in the throat and all down the œsophagus, but had no pain in stomach. Her pulse was 84 and feeble, nearly the same as in the morning, and the temperature of the surface was unaltered. Dr. B. immediately caused her to drink a considerable quantity of warm water, and sent for a full emetic dose of sulphate of zinc. Before the zinc was procured, she had commenced to vomit: but to make sure, it also was administered. The vomiting was now continued and severe, and was further encouraged by additional large quantities of tepid water. The *ejecta* contained, in addition to the water swallowed, a considerable amount of bile, and also a little of the remains of a scanty meal she had taken three hours before; they tasted strongly at first of croton oil. After the vomiting had continued for a quarter of an hour, she began to complain of severe pain in the stomach, not aggravated, however, by pressure. She was ordered an enema, consisting of thirty drops of tincture of opium in a tablespoonful of starch, with directions to have it repeated in an hour.

Dr. B. saw her again at half-past eleven. The bowels had been very freely opened about ten, and again at eleven; but the vomiting had ceased. The pain had considerably abated in the stomach, but was now very severe at the lower part of the bowels; but at neither place was it increased by pressure.

In a day or two she was as well as before the accident.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Recurrent Fracture of the Patella; successful employment, for the second time, of Malgaigne's Hooks. By JOHN H. PACKARD, M. D., of Philadelphia.

In the April number of this journal, I recorded a case of transverse fracture of the patella, which had been successfully treated by means of Malgaigne's hooks. The accident, a fall down a staircase, occurred on the 28th of November last; the hooks were applied on the eighth day, and were retained in place for thirty-one days. By the first of February, the patient was walking in the street, and two months afterwards, as she subsequently told me, she could have waltzed as well as ever she could in her life.

On the 9th of April, Mrs. J. sustained a strain of the left knee (the one previously injured), and on the 10th, catching her foot in the carpet, she felt something give way with a crack in front of the joint. She did not send for me till the succeeding evening, when I found the fracture evidently reproduced, the two portions of the bone being about three-fourths of an inch apart.

For two or three days the limb was simply kept on a pillow, with adhesive strips so applied as to diminish the gap between the fragments. On the 14th, I placed it on a posterior splint, and on the 17th, I re-inserted Malgaigne's hooks, as nearly as possible at the same points as before. The pain caused by the operation subsided in a few minutes, a rag dipped in a mixture of equal parts of lead-water and laudanum being placed on the skin; and the patient lay in perfect comfort during the remainder of the day. At the end of twenty-nine days, the instrument was removed, without having caused any inflammation or irritation. But, whether from a want of due activity in the tissues, or from some other unknown cause, union was not so firm after this second fracture as after the first.

At the present writing, eighty-six days since the instrument was removed for the second time, Mrs. J. walks about with great ease, and with as perfect a sense of security as could well be expected in view of her past experience. When she goes up or down a staircase, a slight halt is perceptible, but otherwise she neither feels nor exhibits any abnormity of gait. The gap between the fragments does not exceed half an inch.

I would call attention to the fact, that when the bone gave way for the second time, it was with a sharp crack, and that when the finger was applied at the seat of injury, the separated fragments were not felt to be smooth and rounded, as if a fibrous bond between them had been ruptured, but abrupt, like those of a common fracture. From these circumstances, and from the closeness of the coaptation maintained by the hooks, my own belief is, that bony union had actually occurred. At all events, the issue of the second course of treatment could hardly have been rendered more successful under any other method of treatment.

(In the description of M. Malgaigne's instrument, given in connection

with the history of this case in the April number of the *Journal*, the thickness of the plates was inadvertently stated to be a quarter of an inch; it is, in fact, not quite one-eighth of an inch.)

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Case of Coccyodynia cured. By A. GODFREY, M. D., of Sonora.—A few weeks ago I met with a case of coccyodynia of ten years' standing, in a woman, about 32 years of age. She suffered very severe pains in the region of the coccyx, whenever she attempted to sit down or to resume the erect posture, or to walk about. She could not lie down in bed or rise from it without the assistance of some one of her family, nor was she able to sit down on and to rise from a chair without taking hold of the back of the chair or some other object. Still, the least painful posture for her was to keep bent to a certain degree, as if in the act of sitting. While sitting on a chair and lying on her bed, she experienced a very disagreeable, dull, burning, and prickling sensation, which was frequently unbearable. She suffered excruciating pains when her body was swayed from side to side; the gluteal and coccygeal muscles of both sides seemed mostly to trouble her, but especially on the right side, to which the coccyx was somewhat drawn. She dreaded to sit down, and dreaded to rise up—dreaded to get into bed, and dreaded to get out of it. As she did not complain when her bowels were being moved, the tendons of the sphincter, and the fibres of the levator ani, did not appear affected, the disease seemed to be mostly confined to the gluteal and coccygeal muscles, and also perhaps to the sacro-sciatic ligaments, which produced traction. The patient states that she cannot tell how the affliction came on, and what caused it. She relates that she consulted a dozen of the best physicians in Tennessee (where she used to live), under whose treatment she successively was for a good while, without deriving any benefit from it, and finally they pronounced her incurable.

Whilst I was attending on a member of her family, she told me her affliction; and the symptoms of coccyodynia, as described in the lectures of Professor Simpson, which I had read in your *Monthly News and Library*, were so striking and plain in her case, that I offered to cure her. I proposed to perform the operation recommended by Professor Simpson, the isolation of the coccygeal bones from the surrounding tissue by means of a tenotomy knife (see *Simpson's Clinical Lectures on Diseases of Women*, Am. ed., p. 216), to which she readily consented. I performed the operation on the 18th day of April last. The relief was effectual, instantaneous, perfect, and I hope permanent. The wound is healed up, and she feels very well. The patient continues to be perfectly free of pains.

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Case of Triplets. By JAS. W. PITTINAOS, M. D.—I was called on the 6th of November, 1860, about half-past four o'clock P. M., to see Mrs. O'C., in labour. This was her sixth pregnancy. When I arrived, the child had been born two or three minutes; it was a girl, and weighed, about four and a half pounds; it was quite vigorous for its size. After having attended to it, I requested the mother to bear down, to facilitate the expulsion of the placenta at the next pain. She did so, and a boy was expelled with the membranes entire. A woman who was present informed me the first child was born in the same condition. This boy weighed nearly three and a half or four pounds. The circulation being feeble, I sprinkled cold water upon the child, and immediately it began to cry. In a few minutes it became very lively; it was weaker than the first.

I waited again for the expulsion of the placenta, but was disappointed, as in a few minutes another boy came forth with the membranes, like the two preceding, entire. This child was remarkably pale and weak. I treated it in the same manner as the other boy, and had the satisfaction to see it improve very much before I left the house. It was, however, the least vigorous of the three.

The placentæ, two in number, were expelled in five minutes after the birth of the last child. The boy and girl first born were attached to the larger placenta, and the last boy to the smaller, which was the last expelled. The children were all doing well when I left the house. For the mother, I ordered the ordinary camphorated solution of sulphate of morphia, two grains to the ounce, of which she was to take a teaspoonful every two hours until sleep was induced.

The expulsion of the children and placentæ did not occupy more than twenty minutes. The woman never had a plural birth before; her husband's sister had twins on one occasion.

Nov. 7th. Saw Mrs. O'C. this morning at ten o'clock. She had passed a comfortable night; this morning, however, she was seized with nausea and vomiting. Great prostration followed, and I found her hardly able to speak. I prescribed for her a half grain of opium, conjoined with one of calomel, every two hours, intending to see her in the evening.

This woman had no milk, but a neighbour who had been recently confined was suggested to me as a person who would nurse the children temporarily. I had, therefore, ordered the woman who acted as nurse to apply to her, but she neglected to do so. The children being naturally weak, having been without food for fourteen hours, and also poorly clad, were cold and debilitated; in fact, the fingers of one were blue with cold. I insisted on their being fed, in no gentle tone, before I left.

I called in the evening to see them, but the gentleman who had been engaged to attend the woman had taken charge of the case. I would have weighed these children, and suggested it to the mother, but she gravely informed me "she never had but one weighed, and all the children were healthy but him, and he had had a *sore neck* ever since." Of course, I urged the matter no further. The parents were wretchedly poor.

11th. I have since learned all the children are dead. The boys died on the 8th—one in the morning, the other at night; the girl died on the evening of the 9th. If these children had been properly cared for by the parents, the result might have been different.

PHILADELPHIA, December, 1861.

DOMESTIC SUMMARY.

Vesico-Vaginal Fistula; Spontaneous Relief. "The American Operation."—[Read before the Obstetrical Society of Boston, by B. E. COTTING, M. D., Cor. Secretary of the Society.]

VESICO-VAGINAL fistula has been justly called a lamentable accident. Its consequences are truly deplorable; the remedy difficult, and, until recently, very uncertain. Spontaneous recovery, a result hardly to be hoped for, is a mere possibility. As an instance of such unexpected good fortune, the following case seems worthy of record.

In January, 1844, the patient, aged 33 years, was delivered of a first child,

after a labour of five days without intermission. On the third day her medical attendant left her, because she would not allow the forceps to be used. After he left, she got no attendance until the last moment of her labour, when she had the assistance of a midwife. During the five days she was not conscious of having passed any urine. Before the removal of the placenta, the water was drawn, and the catheter was used at proper intervals afterwards. On the third day after delivery, however, she found that she had no control over the bladder. After this, all the urine came away involuntarily and without cessation.

Having been called at this stage of the case, I found a sloughy opening from the vagina into the bladder, nearly an inch above the neck of the latter, large enough to admit the tips of two fingers. The catheter passed readily from the bladder into the vagina. The nature of the case, thus clearly made out, was explained to the patient; and she was informed that, after her recovery from confinement, means for permanent relief might be tried with reasonable chances of success. From time to time thorough explorations were made; and the case on the whole was considered a promising one for a surgical operation.

This was in 1844, and I was led to take this view of the matter, because I had been either an observer or an assistant at most of the operations for this accident performed previously, or about that time, by Dr. George Hayward, Sen., of Boston. But the patient resolutely refused to submit to any operation, in spite of earnest and repeated persuasion, and at length declined all further interference; having, from the first, allowed examination very reluctantly. All unusual interest in the case therefore subsided; and for a long time after, nothing was heard of the patient, except that she was in a wretched condition.

Seventeen years having passed, I accidentally, a few days ago, met with this patient, and obtained from her some particulars of her history subsequent to her misfortune.

She had never been pregnant since, though she says that she knows of no reason why she should not have had other children. For three years or thereabouts, after the accident, she was unable to retain the least accumulation of urine, the dribbling having been constant and unceasing. In a word, she suffered all the discomforts and loathsomeness ordinarily attendant upon such accidents. She afterwards began to experience some slight power of retention, and this continued to increase, very gradually, until she had almost or quite acquired complete control over her urine. For the last three or four years, and since her catamenia have ceased, she has been able to retain and pass her water, as she says, "as well as any body." She goes to church and other public places without apprehension, though after several hours she sometimes feels uneasy and fears consequences. She has not, however, suffered from too long retention up to this time. She affirms that she is completely restored, and perfectly well.

On partial digital examination, which she very reluctantly permitted, there were noticeable deep corrugations and contraction about the place of the former opening; and the upper part of the septum appeared to overlap the under. No communication with the bladder could be discovered by the finger; but whether it was completely obliterated, or, if any, how large the opening might be, could not be ascertained with absolute certainty, as she repelled the introduction of the catheter—seizing the instrument and preventing further exploration. At any rate, the injury has been completely repaired, and the patient relieved of her miserable state and restored to a comfortable existence.

In looking up and reviewing a case like the foregoing, which terminated so fortunately, one is naturally led to reflect on the great difficulties formerly attendant on efforts to remedy such accidents, and to call to mind some of the circumstances of the earlier operations performed by Dr. Hayward, particularly those original and scientific devices of his, which have rendered subsequent operations so much more successful; and which, having been accepted by such men as Professor Verneuil and M. Robert, of Hôtel Dieu, have given to the method abroad the name of "The American Operation."

An account of some of these improvements is worth repeating—for their own intrinsic value, and more especially now as they do not seem to be fully appreciated or properly acknowledged by some, whose success, whatever it may be,

should be attributed to the more or less complete adoption of these improvements; and moreover as, in certain localities, they appear to be in danger of being overlaid by accumulating heaps of pretended discoveries and inflated assumptions.

Bearing in mind that when the first case presented itself to Dr. Hayward, in 1839, there had not been a successful operation in this country, and very few anywhere; that little or no assistance in the details of such an operation could be obtained from any source; that troublesome and alarming symptoms had arisen from sutures carried through the walls of the bladder, as previously thought necessary; that danger and even death had occurred from hemorrhage—bearing in mind these and other formidable obstacles on the one hand, and the confessedly slight chance of success on the other, we shall have a better conception of the great service Dr. Hayward rendered. in planning and executing, on strictly scientific principles, a *new*, *safe*, and *successful* method of operating for the relief of this accident.

And first, by passing a large, smooth, and inflexible staff through the urethra beyond the fissure towards the fundus of the bladder, and using this staff as a lever, the pubes being the fulcrum, he showed that the bladder could be brought forward until the fistulous opening came quite within reach and sight. This movement originated with Dr. Hayward. It can be effected without difficulty; and, even before the use of ether as an anæsthetic, caused no very considerable amount of inconvenience or pain. Now, of course, whenever ether is resorted to, the last and only objection is entirely obviated.

The parts having thus been rendered accessible, Dr. Hayward's next and chief improvement consisted in superficially but thoroughly paring the edges of the fistula without cutting deeply into the walls of the bladder, and more particularly in dissecting or splitting up the vaginal membrane around the opening for the distance of a few lines, in order to have broader surfaces to be placed in contact; thereby increasing the chances of adhesion, while the danger of hemorrhage, the result of deeper incisions, was greatly diminished. In fact, the bleeding was in this way reduced to the merest trifle, and the loss of substance, always to be avoided, rendered so inconsiderable that it need not be taken into account.

A third and very great improvement, introduced by Dr. Hayward, was the passing of small sutures; and these through only a portion of the thickness of the pared parts—not through the walls of the bladder. Thus, inflammation of this organ, as experienced by previous operators, was completely averted.

In his first cases, Dr. Hayward removes the stitches a few days after the operations; but having in one instance, at least, which I happened distinctly to witness, reopened an already united wound by pressing too suddenly on the staff in his endeavour to get at the most distant suture, he allowed in some of his subsequent operations the threads to remain until spontaneously cast off. Left in this way, the threads caused no trouble whatever, usually coming away in from seven to ten days. In one instance one remained twenty-eight days without inconvenience or injurious effect—so that this course may be adopted whenever it may be difficult to reach or to remove the stitch.

As so much has been said, from time to time, since the publication[1] of Dr. Hayward's method and success—now on the varied contrivances for securing and retaining the sutures, again on their number and size, and still more on their material, till "lo! a new era dawns"! and we have "the great surgical achievement," "the imperishable discovery" of the silver suture (though unfortunately for our country's vaunted glory in the matter, this "result of a Providential train of circumstances" occurred fifteen years after silver sutures had been in use in England, been advocated there on precisely the same grounds, and even been employed in a successful operation for vesico-vaginal fistula, as reported in vol. xxx. of the London *Lancet*); and as there is no little danger in all this clamour about clamps, buttons, shot, and other equally unimportant mechanical contrivances, that the true scientific principles, which should guide in these

[1] American Journal of the Medical Sciences, Philadelphia, July, 1839, vol. xxiv. p. 283. Boston Medical and Surgical Journal, April 16th, 1851, vol. xliv. p. 209.

operations, may be overlooked or disregarded—it may be well to remark in a word, and it needs but a word, that, wherever union by the first intention is looked for, the edges of the wound must be kept in close contact; and that, if this be skilfully done, it is of far less consequence by what peculiar contrivance it is effected. If sutures are used, more depends upon their proper adjustment, and their having the exact amount of tightening requisite, than upon the material. A metallic suture, of whatsoever or whomsoever's make, if imperfectly secured or too tightly tied, will prove as ineffectual or will cut its way out as certainly as that made of silk or flax. A delicate thread, even of cotton, properly adjusted, will retain its place, cause as little irritation, and leave as small a scar, as we have often had occasion to notice in operations about the face, as the purest silver, the softest iron, or the most polished steel. Let each operator, then, use whichever suture, knot, or fastener he himself chooses or can, in a given case, best manage, just as he would select any particular form of scissors or knife for paring the opening; but let him not forget the principles on which he must depend for success. The former, though perhaps not the best, may answer if adroitly used, but a neglect of the latter will result in inevitable failure. And having, by such a course, been successful, let him not, in his report of the case, as is too often done, wholly ignore the first demonstrator of these principles—an act of simple justice; while magnanimity would suggest an honourable mention.

The position of the patient during the operation is of some consequence, though it may occasionally be varied to suit the convenience, or even the whim, of the operator. Dr. Hayward adopted that of lithotomy, which has many advantages. The fistulous opening, naturally thrown forward by this position, can thus without difficulty be brought by the staff nearly or quite to the os externum, and the subsequent steps of the operation thereby greatly facilitated. In this way too, an assistant, on either side, can with one hand keep the leg in proper position, and with the other separate the labia with a suitable spatula, without being in the way of the operator, who stands in front of the patient. Besides, the patient is in the most comfortable posture for a prolonged operation, and can thus take ether when and as long as desirable.

The catheter for after use, contrived by Dr. Hayward, is, to say the least, quite as good as any of its imitations; while its advantages are the plate which enables it to be secured by a bandage, and the screw which allows the additional portion to be turned in any desired direction, or to be removed at pleasure.

Much more might be added, but enough has been said for the present purpose, which is, simply, a short exposition of the principal improvements, based on which, the operation for vesico-vaginal fistula is hereafter to become one of the successful operations of surgery. Having had the opportunity to be present and to assist in the earlier cases, I can bear testimony to the difficulties encountered, and to the original as well as successful means adopted to surmount them—a grateful testimony to the merited eminence of a faithful instructor and steadfast friend.—*Boston Med. and Surg. Journ.*, July 11, 1861.

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Affection of Vision from Spinal Irritation.—The following cases, related (*Berkshire Medical Journal*, July, 1861) by Dr. A. H. ROBINSON, of Concord, N. H., are highly interesting, though so far as we can judge, from the very imperfect account of the symptoms, the disease from which the patients suffered, seems to be improperly designated amaurosis.

"About two years ago, my attention was called to the case of two young ladies—sisters—who, while attending school at an institution in this county, were both attacked with symptoms of *amaurosis;* which became so severe as to force them to leave school, and live secluded from the light. At the time of my first seeing them, these girls had been some months under treatment, and their health had in a measure improved. *The lower cervical and upper dorsal vertebræ in each were exceedingly tender on pressure;* and as this tenderness abated under local treatment, the pain in the eyes and the intolerance of light subsided; and as the soreness of the spine increased, so did the amaurotic symptoms; and thus the one appeared to be the mark of the severity of the other.

"A few months after seeing these young ladies, my own son, a lad of eleven

years of age, attending one of the schools in this city, began to complain seriously of his eyes. They 'watered' badly, and at times flashes of light darted through them, while the words of his book so ran together that he could not read. The intolerance of light was such as to prevent his enduring a bright sun, or the light of the gas in his room; and the pain in the eyes was so severe, even in the dark, as to prevent his sleeping. His appetite failed, he became emaciated; and the somewhat dilated, but mobile pupils, the drooping eyelids, the pallid countenance, and, in short, the general aspect of the case was so much like the two first, that from curiosity I examined his spine, and found in the same place the same excessive tenderness as in the others. I then took him from school, forbidding his looking at a book, and kept him secluded from all bright light. I applied croton oil to the spine, and gave him blue mass as an alterative, while he took thrice, daily, lupuline and rhubarb, with super-carbonate of soda. This was the main medicinal treatment, and as the irritation of the spine began to abate, the amaurotic symptoms diminished. He could not, however, for nearly or quite a year, read fine print, without the aid of glasses suitable to persons fifty years of age, although words printed in a large, coarse type were readily made out. In about fifteen months after commencing treatment, my son returned to school, and now he can read the finest print without difficulty, and without the aid of glasses, has no pain of the eyes, no unnatural appearance of the countenance, and is able to maintain his rank in his class with fair credit to himself. I consider him cured of his amaurosis."

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Action of Chloroform on the Blood—Probable Causes of its Fatal Effects when inhaled as an Anæsthetic.—Dr. Chas. T. Jackson makes some interesting remarks on this subject (*Boston Med. and Surg. Jl.*, March 28, 1861). Six years ago he published a case of death in a healthy young woman, in whom death was produced from inhaling chloroform, preparatory to the extraction of a tooth. The blood taken from the right side of the heart was analyzed, and was found to contain formic acid in considerable quantities, but no chloroform in an undecomposed state. Microscopic examination of the blood, by Dr. J. Bacon, showed that the blood-globules were withered up in a very remarkable manner. Chlorine was also found to be a component of the blood. It was also observed that this blood did not coagulate, nor did the blood-globules subside, but the whole remained a solution, the blood having a cranberry-red colour, like red ink.

Dr. J. states that this blood has been kept in a vial in his office, exposed to temperatures from the freezing point to above 80° for more than six years, and yet it has not decomposed, nor has a single blood-globule settled to the bottom of the vial, nor has the colour changed in the least.

He now calls attention to these remarkable phenomena, which he thinks appear to throw some light on the chemical action which chloroform exerts on the blood. As he views the matter, he considers the chemical action on the blood by inhaled chloroform to be this: "Chloroform consists of one equivalent of formyle and three of chlorine. Formic acid consists of one equivalent of formyle and three of oxygen. When chloroform is inhaled into the lungs, the oxygen is abstracted from the blood, and combining with the formyle makes formic acid, while chlorine combines with the blood as a substitute for oxygen. Thus a portion of the blood becomes chemically changed, disorganized, and rendered unfit for its vital functions. Then, if that portion of the blood contained in the heart, and large vessels, which we may style the immediate working blood of those organs, is vitiated, so as no longer to be an appropriate stimulus to the most vital organs, sudden death is most likely to take place through cessation of their action. Our only surprise should be that death does not more frequently follow from inhalation of this dangerous anæsthetic agent, for the decomposition I have described undoubtedly always takes place to a certain extent, and to a sufficient degree to deoxidize a considerable portion of the blood, and to charge it with chlorine and formic acid.

"It is well known, that deaths more frequently take place in cases where chloroform is administered in minor surgical operations, or in cases where there is no loss of blood, than in larger and bloody operations.

"This I think admits of an explanation in the fact, that where there is large hemorrhage the vitiated blood, or a portion of it, is removed and is replaced in the vital organs by blood coming from other parts of the body, where it has been but little acted upon by the chlorine—a new working blood being drawn into the large circulating organs, which are thus enabled to continue their proper action, and to supply the brain with its appropriate stimulus. This hypothesis appears to me to cover the cases thus far reported, and I offer it for the consideration of physiologists.

"The great disadvantage under which chloroformization labours, is the introduction of chlorine, as such, into the composition of the blood. This we know to be destructive of the blood-globules, and an actual poison in the system.

"We naturally inquire how this vitiated blood is got rid of, for it evidently never can be re-organized in the circulation. It is undoubtedly thrown off as so much effete matter, by the secernent and excretory organs, and chiefly by the kidneys. Hence, in a large majority of cases, the patients recover with but little damage to their health, though there is always a degree of prostration in most if not all cases of chloroformization.

"It is obvious that we may much diminish the danger in employing this agent, by mixing with it a large proportion of pure ether, say at least three-fourths of its bulk. By this means we not only dilute the dangerous with a perfectly safe anæsthetic agent, but add the stimulus of ether to overcome the deadly, depressing effects of chloroform. The object in allowing any addition of chloroform to ether, was to obtain a less bulky and more powerful agent than pure ether alone, and to afford greater facility in carrying an anæsthetic agent into the field of battle. It was, therefore, recommended by me, and was used in the French and Sardinian armies of the Crimea, with great success and safety, while chloroform, which was employed by the English surgeons, produced many fatal accidents. Still, in cities and at home, or in hospitals, I should always prefer pure washed ether, as it is known to be a safe and sure anæsthetic agent, though it is somewhat bulky. However, from my own experience and observation, I must say I never have known any fatal or even inconvenient effect to result from the employment of the mixture of chloroform and ether in the proportions I have named."

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Inversion of the Uterus.—Prof. JAS. P. WHITE relates (*Buffalo Medical and Surgical Journal*, August, 1861) an interesting case of this to which he was called by Dr. T. T. Lockwood. The subject was in her 23d year, of small stature, and the mother of two children. Nothing unusual had occurred in her previous labours, but Dr. Lockwood had remarked that her pelvis seemed remarkably large, and that the second stage of labour had each time been completed with great rapidity. Dr. L. had been called to the patient in her third labour, on the morning of the 8th of December, 1860. "The first stage of labour, during which there were no unusual symptoms present, had occupied a little more than two hours, the patient not being at all exhausted, and was completed a little after 12 o'clock. At this time the membranes ruptured, a large amount of water was suddenly discharged, and with the next pain the child was thrown entirely into the world. Dr. L. immediately slipped the cord, which was of rather more than ordinary length, and once *around* the *child's neck*, but *not tightly drawn*, over its head. Soon after, as the child was crying vigorously, he tied and divided it, handing over the infant to the nurse.

"His attention was immediately called to the mother, whom he now observed to look pale and to be fainting. Upon passing his hand up between her thighs, without having made the *cord tense*, he discovered a large tumour occupying the entire space, which, upon examination, he found to consist of the uterus, with the placenta attached. He immediately detached the placenta, carried the uterus up into the pelvis, entirely out of sight, where meeting with resistance to further reduction, he gave the woman a large draught of brandy and paregoric, and drove up to my house as rapidly as possible.

"Without loss of time I returned with the doctor to the residence of Mrs. R., whom we found surrounded by her friends, who supposed her dying. The wo-

man was nearly pulseless, with all the symptoms of suffering from severe shock present in an eminent degree.

"We immediately administered more brandy, tinc. opium and ammonia, and as she aroused a little and consciousness partially returned, we deemed it best to proceed at once with efforts to reposit the uterus. Upon introducing my hand into the vagina, I found the fundus of the uterus resting upon the perineum, though *entirely covered by the external organs.* Carrying the hand farther up, the inversion was found to be *complete*, and the organ pretty firmly contracted. The left hand placed upon the supra-pubic region, detected a tumour there, which, although not round like the fundus of the uterus might, through abdominal parietes of moderate thickness, not unlikely be mistaken for the normally contracted uterus in its natural position. Gentle continuous pressure was now made by the intra-vaginal hand, whilst with the one upon the hypogastrium the anterior lip was hooked or held by the fingers, and counter-pressure exerted so as to prevent injury to the utero-vaginal connection. In a few minutes the neck began to be reflected upon the body, and although some dimpling or depression could be felt in the fundus, reduction took place from the neck to the body, and finally the fundus following the body up through the neck to its natural position. It should be remarked that, notwithstanding the relaxation of the system consequent upon syncope, and although not more than an hour or an hour and a quarter elapsed from the time of the delivery of the child to the complete *re-position* of the inverted uterus, the organic contraction was so firm as to require considerable pressure to carry the body through the os, and reflect the organ upon itself.

"The patient was now laid in a comfortable position, with the head depressed and stimulants and opiates freely given. She soon began to revive, the pulse returned to the wrist, consciousness was restored, and at 3 P. M., about two hours after the operation, I left her with Dr. Lockwood. The amount of blood lost was not great, and was, in my opinion, utterly insufficient to account for the depression which immediately succeeded inversion, and continued for some hours after restoration. Indeed it is not unlikely that the patient would have lost her life from shock had not stimulants been freely and repeatedly given. The vital energies were so much depressed—the pulse not to be felt at the wrist—and the countenance was so ghastly that we did not venture to make the least effort at restoration of the inverted organ until she had taken stimulants freely. And yet there had been very little blood lost—for several days Mrs. R. remained very feeble, with frequent pulse, and tender hypogastrium, but gradually convalesced. Her restoration has been complete, and she is now in good health, nursing her infant and attending to her ordinary domestic duties."

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New Suture.—Dr. W. T. S. Cornett, of Versailles, Indiana, describes (*Cincinnati Lancet and Observer*, Aug. 1861) a modification of the suture proposed by Dr. W. L. Atlee. (See No. of this Journal for Jan. 1860, p. 81.) Instead of the gum-elastic ring used by Dr. Atlee, Dr. C. uses a gum-elastic strap, and he prepares needles of steel wire. His directions are as follows:—

"Take the best steel wire, No. 9, such as is used for piano strings, turn an eye on one end, and point the other by means of a file and whetstone; on the eye I make a head with sealing-wax. These pins can be bent with the fingers into any desired curve, to suit irregular surfaces; are sufficiently strong, and can, after their introduction, be easily cut off at the point with wire nippers.

"*Mode of Operating.*—First, pass the pin through the strap near one end, then pass it through the wound, and so soon as the point emerges on the distal side stretch the strap across and apply it on the point of the pin, then push the pin forward until it is arrested by the head coming in contact with the strap on the proximal side of the wound."

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Desquamation of the Cuticle in a Living New-born Child.—Dr. Read said (Boston Society for Medical Improvement) he had attended a woman who was prematurely confined in consequence of having fallen down stairs. The hands and feet of the child were denuded of cuticle, which hung from them in shreds. The child was apparently at the eighth month, and lived twenty-four hours.

There was no other sign of disease. The case showed that desquamation of the cuticle is not always a sign that the fœtus has been dead a considerable length of time.—*Boston Med. and Surg. Jl.*, Sept. 12, 1861.

Removal of Five Inches of the Lower End of the Fibula, from a Boy two years and seven months old; Reproduction of Bone; Cure.—Prof. E. S. COOPER relates (*San Francisco Med. Press*, April. 1861) the following case:—

"Master August. J——, admitted into the Pacific Clinical Infirmary, July 18, 1860, for a disease of the tibia, of ten months' standing. The affection of bone was the result of a very slight injury of the limb, a year previously.

Condition of the parts when operated upon.—The limb was swollen to two or three times its normal size, and at times quite painful. Sinuous openings, leading to the bone, were found in different places.

Operation.—An incision, eight inches long, was made on the outer side of the leg, over the spine of the fibula, through the soft parts covering that bone, and continuing to below the external malleolus. The soft parts were then removed with a sharp bone chisel (which, by the way, is the only proper instrument for this purpose). With this instrument, the soft parts are peeled off from the bone, which may, in many instances, be fully exposed, without any transverse section being made, or risk of wounding adjacent bloodvessels or nerves. And it is the transverse incisions that do the injury in all operations upon the extremities. The mere separation of the tissues amounts to comparatively nothing, let the length of the incisions be what it may in effecting this.

The soft parts being removed, the fibula was fully exposed. It was found much diseased, and in the lower part almost as soft as cartilage. Five inches of the lower extremity were removed, including the external malleolus. The bone was divided above, by a large pair of bone forceps, and its attachments below removed by the chisel.

The bone was somewhat enlarged above the point at which it was divided, but not diseased to a sufficient extent to require removal, it being thought that, by having the part exposed, for the purpose of applying medical substances to it, a final cure would be the result. This plan was accordingly resorted to successfully. A piece of lint was applied to the wound, whence the bone was removed, and a roller put around the limb, as tightly as the patient could well bear it, commencing at the toes. This dressing was kept wet with an evaporating lotion, composed of one part of alcohol and ten of water (the best proportions for our climate), for the first ten days. After which the lotion was exchanged for poultices, which were renewed morning and evening. The lint was removed about the time the poultices were first applied, and the wound permitted to fill by granulations. A weak solution of the tr. iodine was applied in the wound, and over the lower part of the limb. Sometimes this caused pain, and required to be used unremittingly, with other applications, as a weak aqueous solution of ferri potas. tart., black lotion, &c. &c.

The patient took iodide of potassium and ext. of conium the first three months after the operation.

The wound gradually healed by granulation, and the strength of the limb has been slowly restored until the present time, March 30, 1861, eight months after the operation. The little patient walks comparatively well, and is all the time improving, with a prospect of its continuance until a complete cure is effected, with an entire reproduction of the lost bone. The solidity of the parts occupying the place of the lost bone is such as to indicate its reproduction, to a great extent, already. But to those familiar with the immense number of cases in which a large amount of bone has been removed (in California), and afterwards restored by reproduction, the result of this case will not be surprising, but it will be considered rather as a matter of course. While our brethren abroad marvel at the manner in which surgeons of this coast '*play with the bones and joints*,' as they express it, the better informed practitioners of this city regard these almost universally successful results, occurring after the removal of large portions of the shafts of the long bones, as results very naturally to be expected."

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JEFFERSON MEDICAL COLLEGE—1861-2.

The Session will commence on Monday, the 14th of October, with a General Introductory Lecture by one of the Professors. The regular lectures will begin the day after. The Session will terminate on the last day of February.

ROBERT M. HUSTON, M. D., { Emeritus Professor of Materia Medica and General Therapeutics.

CHARLES D. MEIGS, M. D., { Emeritus Professor of Obstetrics and Diseases of Women and Children.

Institutes of Medicine, ...	By Prof. ROBLEY DUNGLISON, M. D.
General, Descriptive and Surgical Anatomy, .	" JOSEPH PANCOAST, M. D.
Chemistry, ...	" FRANKLIN BACHE, M. D.
Institutes and Practice of Surgery, . .	" SAMUEL D. GROSS, M. D.
Materia Medica and General Therapeutics, .	" THOMAS D. MITCHELL, M.D.
Practice of Medicine, ...	" S. HENRY DICKSON, M. D.
Obstetrics and Diseases of Women and Children,	" CHARLES D. MEIGS, M. D.

Demonstrator of Anatomy, . . ELLERSLIE WALLACE, M. D.

Clinics will be held regularly during September; and every Wednesday and Saturday in October, and during the course, Medical and Surgical cases will be investigated, prescribed for, and lectured on before the Class. During the year ending March the first, 1861, a vast number of medical and surgical cases were treated, and several hundred surgical operations were performed; among them many of the most important.

The lectures are so arranged as to permit the student to attend the clinics of the Pennsylvania Hospital, and the Philadelphia Hospital.

On and after the 1st of October, the dissecting-rooms will be open, under the direction of the Professor of Anatomy and the Demonstrator.

FEES.

Matriculation, which is paid only once, ...	$ 5
To each Member of the Faculty $15, ...	105
Graduation, ...	30

ROBLEY DUNGLISON, M. D.,
Dean of the Faculty.

HARVARD UNIVERSITY.

MASSACHUSETTS MEDICAL COLLEGE.

The annual course of Medical Lectures of Harvard University will commence at the Massachusetts Medical College, in North Grove Street, Boston, on the first Wednesday of November, 1861. The regular course will be as follows:—

Obstetrics and Med. Jurisprudence,	by Professor D. HUMPHREYS STORER, M. D.
Morbid Anatomy, ... "	" JOHN B. S. JACKSON, M. D.
Clinical Medicine, ... "	" HENRY I. BOWDITCH, M. D.
Anatomy and Physiology, . . "	" OLIVER W. HOLMES, M. D.
Theory and Practice of Medicine,	" " GEORGE C. SHATTUCK, M D.
Surgery, ... "	" HENRY J. BIGELOW, M. D.
Chemistry, ... "	" JOHN BACON, M. D.
Materia Medica . . . . "	" EDWARD H. CLARKE, M. D.

Demonstrator, DAVID W. CHEEVER, M. D.

Clinical, Medical, and Surgical Instruction will be given at the Massachusetts General Hospital, with Surgical Operations.

Collateral special medical instruction will also be given at the Hospital by Lectures and otherwise, by Drs BOWDITCH, ABBOT, and ELLIS.

Abundant material is afforded for the study of Practical Anatomy. The Room devoted to this department is open day and evening, and lighted by gas.

Fees for the Lectures, $80; Matriculation fee, $3; Graduation fee, $20.

Good Board can be obtained at $2 50 to $5 00 per week. Boarding places provided on application to the Janitor at the College.

Students are requested, upon coming to Boston, to call upon the Dean.

D. HUMPHREYS STORER, *Dean of the Faculty.*

July 1, 1861. *No.* 132 *Tremont Street, Boston.*

BOYLSTON MEDICAL PRIZE QUESTIONS.

The Boylston Medical Committee, appointed by the President and Fellows of Harvard University, consists of the following Physicians:—

Edward Reynolds, M. D.	J. B. S. Jackson, M. D.	Charles G. Putnam, M. D.
John Jeffries, M. D.	J. Mason Warren, M. D.	Morrill Wyman, M. D.
S. D. Townsend, M. D.	D. H. Storer, M. D.	Henry J. Bigelow, M. D.

At a meeting of the Committee on Wednesday, August 7th, 1861, a premium of Sixty Dollars, or a gold medal of that value, was awarded to Richard M. Hodges, M. D., of Boston, for the best dissertation on the question—
Excision of Joints.

No premium was awarded for either of the Dissertations on the question—
Diagnosis and Treatment of Chronic Pleurisy.

The following are the questions proposed for 1862:—

1. *How far does the Microscope assist us in Surgical Diagnosis?*
2. *On Nausea and Vomiting, as Symptoms; under what circumstances do they occur, and what indications do they afford as to the seat and character of disease?*

Dissertations on these subjects must be transmitted, post paid, to Edward Reynolds, M. D., *on or before the first Wednesday of April*, 1862.

The following are the questions proposed for 1863:—

1. *On Trephining the Skull for Injury or Disease.*
2. *On Leucocythæmia.*

Dissertations on these subjects must be transmitted as above, *on or before the first Wednesday* of April, 1863.

The author of the best dissertation considered worthy of a prize on the subjects for 1862, will be entitled to a premium of *sixty dollars*, or a gold medal of that value, at his option.

The author of the best dissertation considered worthy of a prize on either of the subjects proposed for 1863, will be entitled to a premium of *ninety dollars*, or a gold medal of that value.

Each dissertation must be accompanied by a sealed packet, on which shall be written some device or sentence, and within which shall be inclosed the author's name and residence. The same device or sentence is to be written on the dissertation to which the packet is attached.

The writer of each dissertation is expected to transmit his communication to the Chairman of the Committee, in a legible handwriting, within the time specified.

All unsuccessful dissertations are deposited with the Secretary, from whom they may be obtained, with the sealed packet unopened, if called for within one year after they have been received.

By an order adopted in 1826, the Secretary was directed to publish annually the following votes:—

1st. That the Board do not consider themselves as approving the doctrines contained in any of the dissertations to which premiums may be adjudged.

2d. That in case of publication of a successful dissertation, the author be considered as bound to print the above vote in connection therewith.

J. MASON WARREN,
Secretary.

UNIVERSITY OF PENNSYLVANIA.

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NINETY-SIXTH SESSION, 1861 AND '62.

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